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United States
Department of
Agriculture

Forest Service

Pacific
Northwest
Region

November 1988



Managing Competing and Unwanted Vegetation

Final Environmental Impact Statement

Appendix I/A Public Participation and Consultation



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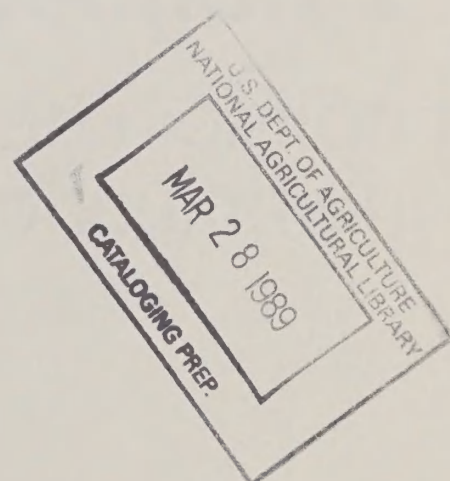
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Appendix I/A

Public Participation and Consultation

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Appendix A

Public Participation and Consultation

1. Introduction

The purpose of this appendix is to provide a framework for public participation and consultation throughout the project lifecycle. It outlines the principles, objectives, and methods for engaging stakeholders and ensuring their views are taken into account.

Public participation is a key element of good governance and is essential for the success of any project. It involves the active involvement of citizens, community groups, and other stakeholders in the decision-making process. This section describes the various ways in which public participation can be achieved, from formal consultations to informal dialogue.

The following sections provide a detailed overview of the public participation process, including the identification of stakeholders, the selection of appropriate methods, and the implementation of consultation activities. It also discusses the importance of transparency and accountability in public participation and provides guidance on how to monitor and evaluate the effectiveness of participation efforts.

By following the principles and methods outlined in this appendix, project teams can ensure that public participation is a meaningful and effective part of their work. This will help to build trust, improve decision-making, and ensure that the project meets the needs and expectations of the community it serves.

The following sections provide a detailed overview of the public participation process, including the identification of stakeholders, the selection of appropriate methods, and the implementation of consultation activities. It also discusses the importance of transparency and accountability in public participation and provides guidance on how to monitor and evaluate the effectiveness of participation efforts.

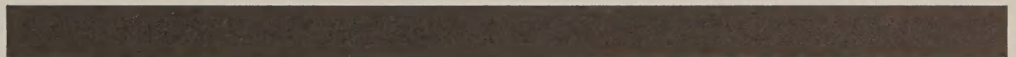
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CLAYDON HILL
2017-01-01

Appendix I/A

**Public Participation
and Consultation**



Section 1

**Public Participation
and Consultation**

Public Participation and Consultation

Introduction

Many people and organizations were involved in the development of the vegetation management environmental impact statement. Those who actively participated made valuable contributions to almost every aspect of the process.

Setting high goals for active participation by the public, working with people early and continuously in the process, developing cooperative agreements with state and national agencies, and seeking expert technical and scientific review were all part of the strategy for involving everyone with an interest in the vegetation management program.

What This Appendix Contains

In this appendix we describe in detail our public involvement activities in constructing this Environmental Impact Statement (EIS). The appendix reviews activities which were initiated during preparation of the draft EIS and carried through to the final EIS. It also describes the formal comment period and how we responded to the many comments we received.

In addition, we discuss the role of the nine cooperating agencies and the extensive review and analysis that went into both the draft and final EIS. We list, by issues, comments people made during the formal comment period along with our responses to those comments and locations in the EIS where more information can be found.

This appendix also contains copies of a wide sample of the letters we received, as well as copies of the letters from federal and state agencies and elected officials. Though many letters we received were not duplicated here, the ones included in this section reflect the flavor, scope and complexity of the responses to the draft EIS.

Public Involvement Goals: "Turning Over a New Leaf"

Issues relating to vegetation management have long been of concern to many people. Over the past years, our interaction with interested and concerned people on vegetation management issues was often controversial and full of conflict. The result of that era was a 1984 court injunction prohibiting the use of herbicides until further analysis was completed by the Forest Service.

In keeping with this approach, early meetings to identify the best strategies for incorporating public participation in vegetation management decision-making were held with people known to have high interest in it.

Key players in these early meetings included individuals active during the 1983-84 litigation as spokespersons for their respective organizations, who also had an interest in working toward mutually acceptable solutions. Among them were representatives of the Northwest Coalition for Alternatives to Pesticides (NCAP) and Oregonians for Food and Shelter (OFS). Two other key groups included the Western Washington Toxics Coalition and Washington Friends of Farms and Forests (formerly Washington Pest Management Council).

During the course of the initial meetings, a variety of strategies for involving the public throughout the development of the EIS were worked out. They included:

- periodic planning progress report mailings;
- leadership by NCAP and OFS in coordinating involvement of the segments of the public they represent;
- issue workshops for the Forest Service hosted by NCAP and OFS;
- assistance to the Forest Service interdisciplinary team by environmental and business community work groups;
- a special outreach program to reach Forest Service employees; and
- contacts and close coordination with other interested organizations.

Throughout the development of the draft and the final EIS, these tools and techniques were used to keep people informed, provide opportunities for feedback and to involve people in the entire analysis.

During the life of this EIS process, many public involvement activities took place.

- We published seven "Requests for Participation" with information on progress and current status.
- We held two issue workshops sponsored by NCAP and OFS, and attended by line and staff officers from the Region.
- Three working groups (NCAP, OFS, Oregon Society of American Foresters) worked for over 12 months with the interdisciplinary team.

- Forest Service employees received all mailings and participated in special briefings.
- Over 100 other organizations were contacted and invited to participate in the analysis. Many of them did.

Public Comment Period

The Draft EIS was released on October 15, 1987. The National Environmental Policy Act (NEPA) requires that all environmental analyses be open to public review. Interested individuals may obtain copies of the environmental impact statement and respond to the agency with their thoughts and comments.

According to NEPA regulations, a 45-day public comment period is required for the draft EIS. We decided on a longer 90-day review because of the broad scope and complexity of this particular document. A 30-day extension was granted in January, 1988, bringing the total review time to 120 days (October 15, 1987, through February 15, 1988).

Role of the National Forests During the Comment Period

The nineteen National Forests in the Region played a major role in the public comment period. The Forests established mailing lists, held open houses, issued news releases, sponsored workshops and gave briefings. As a result, they identified local issues and concerns, additional, key local people were identified, and existing relationships were strengthened. Many people who would not have been reached otherwise were informed about the development of the new Regional vegetation management program.

Regional Coordination

At the regional level, we established coordination with federal and state agencies, and statewide interest groups. We produced an eleven minute videotape about the draft EIS for use by the Forests and the public. Summary materials were prepared, and the interdisciplinary team worked closely with special technical review groups set up by organizations to review the draft EIS.

What About All the Mail?

We received over 5,000 responses to the draft EIS from 29 states and British Columbia. The responses came from individuals, organizations, associations, elected officials, and state and federal agencies. The majority were well-reasoned, thoughtful letters. These were valuable contributions to the final EIS.

As part of our commitment to working closely with people in the

planning process, we responded to the letters we received. Using personal letters, we thanked people for their response, acknowledged points made, clarified information, and let them how we would be using their information in developing a final document.

Analysis of comments is one of the key activities of the EIS process. After all the work is done on a draft statement, the team finally gets to read what interested people think about the product and the process. Sometimes it is gratifying; sometimes it is painful. It is nearly always helpful.

Some people addressed broad, sweeping issues while others commented on specific technical issues. Still others did a good job of editing. It is obvious that people care a considerable amount about how we propose to manage vegetation on the National Forests.

We received 4,929 responses to the draft EIS. The majority of responses were submitted by individuals acting on behalf of themselves or their families. The next largest groups of respondents were conservation or environmental groups, timber industry businesses, associations and unions, and county officials.

Analyzing the Comments

Who Responded?

Table I-1

Who Responded by Group Affiliation

Group	Number of Responses
Individuals and Families _____	4707
Conservation/Environmental Groups _____	51
Timber Industry Businesses _____	35
Associations and Unions _____	3
County Elected Officials _____	25
Businesses or Business Groups _____	20
Federal Agencies _____	9
State Agencies _____	9
State Elected Officials _____	9
City or Municipal Agencies _____	7
Civic Groups _____	7
Other _____	5
City Elected Officials _____	3
Hunting and Sporting Groups _____	3
Academia _____	2
Riding and Hiking Groups _____	2
Indian Tribes _____	2
Professional Societies _____	1
Total	4,929

The public response to the draft EIS included original letters and cards, form letters, petitions, and resolutions. Form letters and printed post cards made up the majority of the responses. Six different form letters were identified. Original letters and cards made up the second largest group of responses, followed by the response form provided by the Forest Service.

Approximate Breakdown of Comments:

1,400 individually written letters;
3,500 form letters including:
3,000 from Oregonians for Food and Shelter
400 from Southern Oregon Timber Industries Association
20 from Earth First!
80 miscellaneous

4,900 total individual responses

74,000 individual comments came from the letters (an average of 15 comments per letter). Of these, 86 percent of these comments came from the form letters, that is, the same 15 comments 3,000 times. Another 14 percent of the comments came from individually written letters.

There were three petitions (with 31, 52, and 73 signatures). Responses came from 29 states, but most were from Oregon (4,100 responses) and Washington (600 responses).

**The First Level of
Content Analysis
(mechanical
sorting of letters)**

Each response was given a unique number and categorized according to its form (letter, form letter, or card). Form letters were grouped together by type. This was done to facilitate the coding and data entry of similar responses.

Each letter was read at least three times by a coder. The first reading gave the coder an overall feeling of the respondent's concerns. In the second reading, the coder selected and highlighted comments that expressed the writer's feelings, and opinions. In the third reading, the coder categorized the comments according to a set of numeric codes, and decided in which database to enter the comments.

Comments were sorted into three categories. Simple comments, the essence of which could be completely captured with the numeric codes, were put into the numeric database.

More complex but relatively short comments which could not be completely captured with the numeric codes were coded for inclusion

in the short comments database.

Long and very complex comments were coded to indicate that an interdisciplinary team member should read the whole comment or the entire letter.

After the comments were all sorted by issue (the seven issues identified in the draft EIS), the interdisciplinary (ID) team analysis began. Each ID team member became an “issue manager” responsible for categorizing comments under his or her particular issue into logical subgroups. These subgroups were summarized into general “comment points” and recommendations for dealing with them were developed and presented to the entire ID team.

The team used colored index cards on an 8' by 28' tack board wall to keep track of the many comments received. Rows of index cards were pinned on the board under each issue category. A different colored card listed the recommendation made to the team. A third card identified the final decision on the recommendation. A fourth card listed where changes would take place in the EIS.

In developing the comment and response section of this appendix, we converted the index cards on the wall to text for this appendix. The wall proved to be a valuable tool for tracking comments and changes, and for displaying information.

A substantial number of comment points had potential for large, sweeping changes in the document, while many others were smaller points of clarification or correction.

All of the comment points (and our responses, together with identification of sections changed) are listed in the remaining sections of this appendix. We summarized comment points with the potential for causing large changes and listed them in the text-graphics preceding each chapter and Appendix A.

After reading and analyzing the sorted comments, the interdisciplinary team identified thirteen major areas for decision and resolution. These broad areas provided the focus and scope of the final EIS. The following paragraphs briefly summarize what people said and identify the thirteen issue areas.

The document is:

well done; very readable; too big; confusing in places (especially risk, and social and economic effects); in need of more analysis; not written as a scientific document.

The Second Level of Content Analysis (interdisciplinary team review)

What People Said: The “Thirteen Questions”

The process:

Has been excellent and comprehensive; violates NEPA with three preferred alternatives; does not reflect a true “no action alternative”.

The largest issues now are:

The potential reduction of timber harvest and the implications of herbicide restrictions. (Many respondents felt that the DEIS over-emphasized risk and understated allowable sale quantity effect to appease a minority.)

The potential threat to humans and the environment if herbicides are used freely. (Many respondents felt that the DEIS understated the risk so that timber can continue to be sold.)

The most popular alternatives:

Alternative B, and B + D: let the individual Forest do its own thing; emphasize prevention.

Alternative D, and D + E: prevention and herbicides as a “last option”.

Alternative C: no vegetation management at all.

Alternative A: vegetation management without the use of herbicides.

After reading and analyzing the sorted comments, the interdisciplinary team identified the thirteen major question areas we referred to earlier. These broad areas provided the focus and scope for development of the final environmental impact statement (FEIS).

1. What will the Record of Decision cover?
2. How will coordination with other agencies work? Who will do what?
3. What will our policy be for the use of herbicides in municipal and domestic watersheds?
4. How can we improve our discussion and analysis of wildlife effects?
5. What can we do to improve the presentation of human health analysis?
6. What are the expected short-term effects, Forest-by-Forest, on

timber allowable sale quantity effects?

7. How can we improve our discussion and analysis of noxious weeds?
8. Will we be changing or adding alternatives?
9. What constitutes effective implementation?
10. How do we achieve adequate monitoring?
11. How do we define integrated pest management and fit it into the FEIS?
12. How do we incorporate what we have learned about risk into decisions (both Regional and field-level) concerning vegetation management?
13. How will we integrate the FEIS into Forest plans?

The responses to these questions and the many other suggestions and questions regarding issues of a narrower scope are included in the last sections of this appendix.

After the comments were summarized and analyzed, we felt that it was important to share the analysis with people who had been involved up to this point. We published the seventh “Request for Participation” summarizing the thousands of comments received.

As the team began responding to comments and initiating new analyses, a concentrated effort was made to keep people informed of our progress. The team formulated Alternative H (a new alternative) and made decisions on other changes and new material to be added. Personal briefings were held with the working groups and other interested organizations. We also briefed review teams from the States of Washington and Oregon, and made contact with congressional representatives interested in the analysis.

Interagency coordination for development of the new vegetation management program was a specific issue identified during scoping. Many agencies have been involved in various aspects of the EIS, and several agencies were designated as formal cooperators.

Designation of cooperating agencies is a provision of NEPA that emphasizes interagency cooperation early in the EIS process. The lead agency (in this case, the Forest Service) may request any other Federal

Developing the Final EIS

Cooperating Agencies

agency with jurisdiction by law (or with special expertise related to an environmental issue) to be a cooperating agency.

State or local agencies with similar qualifications may also become operators through agreement with the lead agency. Agencies which cooperated in development of the vegetation management EIS were:

Environmental Protection Agency, Region X
Oregon State Department of Environmental Quality
Oregon State Department of Agriculture
Oregon State Department of Transportation
Oregon State University Extension
Washington State Department of Agriculture
Washington State Department of Transportation
Washington State Department of Natural Resources, Forest Lands
Division
University of Washington, School of Public Health and Community
Medicine

Review and Analysis

Before publishing the draft EIS, two "rough draft" reviews were held that included review by cooperating agencies, working groups and internal staff. After the public response was received and evaluated, changes were made to improve and clarify analyses, prepare new analyses, develop a preferred alternative, and correct scientific and factual information. In addition, efforts were made to better summarize information and more clearly portray results and conclusions.

The supporting scientific information for the EIS is extensive. Both the University of Washington and Oregon State University have made valuable contributions to the review and analysis used in formulating the EIS.

Scientific Review of Human Health Analyses

Two appendices (D and H) in the draft EIS analyzed human health and risk. Appendix D was a pesticide risk analysis completed by LaBat-Anderson, Inc. for the Bureau of Land Management and the Forest Service. The University of Washington, School of Public Health and Community Medicine provided additional expertise in the field of toxicology and public health. Through this arrangement, a review of the Labat-Anderson risk analysis was conducted and a systematic evaluation of available information on herbicide toxicity was developed (H).

During the public comment period, an extensive peer review program was initiated to evaluate the human health analyses. This

program was commissioned through Oregon State University and the University of Washington.

Eleven eminent toxicologists and scientists from throughout the United States reviewed the technical appendices and the human health analysis in the EIS. Their reviews and our responses are included in the comment section in this appendix.

Prior to issuing the draft EIS, rough drafts of the DEIS and silviculture appendices were reviewed through cooperation with Oregon State University Extension and the College of Forestry. Reviewers included:

- John R. Walstad, Ph.D., Professor of Forest Science, Leader of the FIR Program;
- Steven Radosevich, Ph.D., Professor of Forest Science, Leader of the CRAFTS Vegetation Management Research Cooperative;
- Michael Newton, Ph.D., Professor of Forest Ecology and
- John Tappeiner, Ph.D., Professor of Forest Management.

The cooperative agreement with Oregon State University Extension was maintained after publication of the draft EIS and a critical review of the silviculture and timber yield portions was again conducted. The review is found in the Technical Reviews section of this appendix.

Many people have high interest in vegetation management issues. Many made valuable contributions. We were highly committed to giving people opportunities to contribute to the draft and to the final EIS. We are also committed to continuing an open process throughout implementation of the Pacific Northwest Region's program for vegetation management.

Critical Reviews of the Silviculture Analysis

Summary

Appendix I/A

**Public Participation
and Consultation**



Section 2

**Comments Proposing
Major Changes and
Our Responses**

Comments Proposing Major Changes and Our Responses

Introduction

This section contains the comments shown in the boxes at the beginning of each chapter. Comments in the section labeled “What Reviewers Said” are summations of public responses in the same general subject area. These suggestions would cause major changes in the document if they were augmented. The section called “Our Response” tells what we did in response to the comments. Where we made major changes, we provide guidance for finding those changes in the FEIS. In some where we determined that we would not make a suggested change in the EIS, we give the logic behind not making the change.

Chapter I

What Reviewers Said

Use of the Forest Plan preferred alternatives and incorporating them in the analysis meant that the decision to select the Forests' preferred alternatives had already been made and mistakes in them affect the vegetation management planning process.

Our Response

We provide more information in Chapter I about how we are using the Forest Plan preferred alternatives. We do recognize that not all will become "selected" alternatives. Even so, the preferred Forest Plan alternatives provide us with the best estimate of future management activity levels. We recognize that the alternatives identified as preferred in the Draft Plans may not be the selected alternatives in the Final Plans.

Chapter II

What Reviewers Said

People suggested that we should:

1. Re-examine the relationship between long-term sustained yield and allowable sale quantity. Some felt that a Forest-by-Forest examination would be the most desirable way to calculate the quantity and PNV implications of the alternatives.
2. Explain the reason for a floating budget, particularly with regard to Alternatives A and D.
3. Include economic effects of various taxes.

Our Response

1. We developed Forest-by-Forest Allowable Sale Quantities and have included them in the FEIS. They are used in calculations for jobs and payments to local governments.
2. We now explain the "floating budget" concept in developing Forest data.
3. Regulations relating to the National Environmental Policy Act recommend not including the effects of taxes in analysis. We have cited this reference in the text.

4. Use a different demand curve.

5. Change stream buffer widths. (Some respondents wanted them smaller, some wanted them larger.)

6. Clarify the logic for the extrapolation of growth and yield effects data to make a region-wide estimate.

7. Describe existing silvicultural efforts to incorporate preventive strategies and the use of natural systems to deal with competing vegetation.

8. Include a monitoring plan in enough detail to show how management objectives, mitigation measures, and targets can be achieved over time.

9. Define the term "last option".

10. Explain the "risk indices" for herbicides and smoke used in Chapter II.

4. We used the same curve, but material on demand functions and real price trends has been added to the section on Comparing the Cost/Benefit Dimension of the Alternatives.

5. Rather than change and rely on a single buffer width, we added a mitigation measure that requires site specific analysis to set buffer widths for herbicide application projects

6. A discussion of extrapolation methodology is included in the Comparing the Alternatives section under Comparing the Effectiveness of Techniques Dimension of the Alternatives.

7. We cover this subject in the Five Steps in Managing Unwanted and Competing Vegetation section.

8. We expanded the monitoring plan in this chapter.

9. We wanted public response to help us develop a policy for use of herbicides in our preferred alternative. Our preferred alternative states that herbicides will be used only when necessary: when other tools won't get the job done or would have unreasonable costs.

10. The risk indices were confusing. We do need a way to compare risks between alternatives. Since no absolute measure is possible, we now use acres treated by method as a relative indication of health risk.

Chapter III

What Reviewers Said

1. The whole noxious weeds section is not well developed, and not enough information is presented.
2. Not enough detail is displayed in the wildlife section.

Our Response

1. We rewrote the entire section on noxious weeds, adding detail and depth to the information presented in Chapter III and especially in Chapter IV. Information is also provided in Appendix G.
2. We revised the material in the wildlife section and added many details.

Chapter IV

What Reviewers Said

1. Local timber harvest level adjustments predicted for the first Forest Plan decade should be presented.
2. Forest level timber harvest effects (ASQ) and economic effects are needed to display local impacts.
3. Replace the IMPLAN model with a more complete, up-to-date economic model.

Our Response

1. To address first decade timber harvest levels, we developed and added allowable sale quantities (from the proposed Forest Plans). We also show potential yield estimates from existing plans. The summary of the data from each Forest is shown under Timber Yield Effects.
2. We now include a summary of Forest-by-Forest timber harvest effects. We are also using Forest-by-Forest allowable sale quantities to represent local economic effects. See Chapter IV.
3. We believe the IMPLAN model is the only appropriate one to use in this analysis. We stayed with it but explained why in text. See Appendix B.

4. A serious basic flaw in the entire VMDEIS is the reliance and incorporation of the preferred alternatives from the as yet, uncompleted Forest Plans for the 19 National Forests in the Region.

5. The whole noxious weeds section is not well developed and not enough information is provided.

6. Risks to wildlife should be displayed. Effects on wildlife are underplayed and invertebrates are neglected.

7. Buffer widths are too wide. Buffers are not big enough.

8. There should be no spraying of herbicides in municipal, domestic, or fish hatchery supply watersheds.

4. Preferred alternatives from the draft Forest Plans provide one estimate of a Region-wide likely future. We decided to stay with the data from Forest plan preferred alternatives for many analyses. Where current plans may provide different effects, analysis was conducted using both current plan data and preferred alternative data under the new plans. One example is the predicted change in allowable sale quantity for each National Forest, shown in tables in the Timber Yields section in Chapter IV.

5. The noxious weed material is discussed in more detail in the Rangeland Vegetation sections of Chapters III and IV.

6. We revised the Wildlife and Wildlife Habitat sections and added a wildlife appendix (Appendix J) that discusses effects of herbicides, wildlife risk, and available literature. The Diversity section also contains information regarding the importance of food chain components to the overall ecosystem.

7. Neither our analysis nor public comment provided any rationale to change the buffer widths. However, we have provided a more complete explanation concerning buffers in Chapter IV, Water Resources; herbicide section.

8. We think there will be times when herbicides should not be used in these areas, and times when some of them could be. Therefore, we developed a new policy that requires a site specific analysis (including a project risk assessment) for each project. See Chapter II.

- | | |
|---|--|
| <p>9. Add more clarification of the relationship of the Vegetation Management EIS Forest Land Management Plans, and the Regional Guide. The specific concern is the use of even-aged systems in timber management.</p> <p>10. The treatment of the whole human health risk topic is too technical and confusing.</p> <p>11. The Human Health section of Chapter IV is very difficult to understand.</p> <p>12. It is not clear how you will use the findings of the two human health risk assessments in everyday vegetation management operations.</p> | <p>9. We added clarification of the relationship of the EIS, Forest Plans, and the Regional Guide to the Timber Yields section. This material includes policy guides on even-aged management and clearcutting.</p> <p>10. We developed a new clear, brief summary of human health risks. It is written in everyday language and appears as a stand-alone appendix to the FEIS. It is called the Characterization and Management of Risk.</p> <p>11. The explanation of human health effects has been replaced by the clearer material and tables from the new human health risk summary.</p> <p>12. The FEIS now shows a direct connection between the risk assessments in the appendices, the conclusions about human health effects in Chapter IV, and the management of risk through the mitigation measures in Chapter II.</p> |
|---|--|

Appendix A

What Reviewers Said

1. Explain differences in predicted timber yield effects between this EIS and those done by the Pacific Southwest Region of the Forest Service (Region 5) and the Bureau of Land Management (BLM) of Oregon.

Our Response

1. We added a description of the coordination efforts among agencies and the different conclusions regarding timber yield predictions among Regions 5 and 6 and the BLM in Oregon. A summary of a coordination meeting among the agencies is included in the Timber Yield Effects section.

2. The logic for extrapolation of growth and yield results to the Region--and the use of only full-yield suitable lands--needs clarification.

3. More detailed information is needed on assumptions and methodology used for growth and yield analysis.

4. Conduct a sensitivity analysis to establish some confidence limits or reliability of the long-term sustained timber yield estimates.

2. We clarified and redefined the process for extrapolation of timber yield results across the Region. We included our rationale for using only full yield suitable lands.

The extrapolation process is discussed in Chapter II, Technique Effectiveness section, in Chapter IV, Timber Yields section and in Appendix A, Subregional Effects on Timber Yields section.

The logic for using full yield suitable lands is in Appendix A, Stratification and Methodology section.

3. Material added to Appendix A provides a clear decision path to the conclusions. The addition discusses major assumptions, use of key studies, prescription details, and growth simulation results. We added two tables covering prescriptions and simulations.

4. We conducted a sensitivity analysis based on existing data to establish reliability of estimates. A summary of these findings is located in the back of Appendix A.

Appendix I/A

Public Participation and Consultation



Section 3

Comments and Responses by Issue

Comments and Responses by Issue and Resource

Issue: Human Health

This section contains individual comments from letters on the Draft EIS sent during the public comment period. The comments are organized by issue. The first column lists the comments. The second column lists our responses to them. Where table or page numbers appear, they refer to information in the draft EIS.

Note that there are three groups of human health comments and responses: those for public comments prepared by LaBat Anderson, Inc.; for public comments prepared by the University of Washington; and for comments submitted by the two working groups—Oregonians for Food and Shelter (OFS), and Northwest Coalition for Alternatives to Pesticides (NCAP)—also prepared by the University of Washington.

Introduction

Comments and Responses to Public Comments on Human Health (LaBat-Anderson, Inc.)

Comment

The risk assessment is faulty because it does not adequately address the issue nor examine the risk of inert ingredients in the herbicide formulations, which may constitute a significant portion of the toxic burden.

The importance of safety in reducing worker exposure, and therefore risk, is greatly understated in the EIS. Risk assessments should assume that reasonable mitigation measures are taken. Recommendations for risk management can then be developed to demonstrate conditions for implementation of the alternatives.

Response

In response to these concerns, we have added a section on inert ingredients in the FEIS. The Forest Service has received from the EPA a list of the inert ingredients of concern (EPA list 1) or suggestive of concern (EPA list 2) that are present in the herbicide formulations proposed for use. We are committed not to use any herbicide formulations containing any inert ingredients on these lists without a detailed analysis of their effects on human health. The discussion of inert ingredients in Appendix D, Section 3, has been expanded.

The Forest Service has worker safety training policies and plans in place. In addition, the FEIS includes direction for projects to establish Human Health Risk Management Plans. In the risk assessment, appropriate mitigation measures were considered in routine scenarios. To avoid underestimating risks, the use of protective clothing was not considered, because that practice is not consistently followed by all workers. See Chapter II, Mitigation Measures, for more information. The historical data base is predominately for worker exposures where protective clothing was not worn.

Based on the information presented, it seems that any use of herbicides would be based on incomplete evidence of safety. Therefore, in assessing risks, the Agency must assume that any missing research would demonstrate the worst possible results.

While there will always be uncertainty regarding the potential of these herbicides to cause health effects, there is a large body of data available. We have clearly indicated where there are data gaps and have used accepted scientific practices to extrapolate from these data to give some estimate of potential health effects. A range of exposures, from realistic to worst case, and a number of scenarios that examine routine and catastrophic events have been used to account for uncertainties in toxicity and exposure. In addition, we have consistently made conservative assumptions. Please review the summary on "Characterization and Management of Risk" and the Chapter IV "Human Health" section.

Risks to health have been severely underestimated. Regardless of the scenarios' titles, only low- and no-impact situations are addressed. Reasonably possible catastrophic- and medium-impact events are not acknowledged. What if the assumptions to minimize risk to the public prove to be incorrect?

We have incorporated a number of worst case assumptions in our exposure scenarios (for example, none of the herbicide degrades; workers are not wearing protective clothing). In addition, the EIS does examine a number of low-probability, high-impact events, such as the crash of a helicopter or the spill from a truck into drinking water sources.

The relationship between carcinogenicity and mutagenicity is misinterpreted, specifically in the qualitative and quantitative prediction of potential mutagenicity inferred from carcinogenicity results. Current understanding of cancer induction is that not all carcinogens are mutagens and that direct genetic damage is associated with some cancers.

The EIS thoroughly discusses the risks of using herbicides, giving the impression that there are many serious associated risks, while nonherbicide methods are discussed in such a way that it suggests they have no serious health risks. All tools should receive the same degree of scrutiny if the appropriate risk management alternatives are to be developed.

This EIS used the most conservative approach when assessing the toxicity of the chemicals. If data were incomplete for mutagenicity, or if existing data were indicative of weak mutagenicity, then the assumption was made that the chemicals were mutagenic. In the case of incomplete data, the results of carcinogenicity tests were used to estimate mutagenic risk.

We have revised the EIS to include a more detailed discussion of risks of nonherbicidal methods. Please see Chapter IV and the separate summary on "Characterization and Management of Risk."

The authors assume that all oncogenicity data in laboratory animals should be equally weighted for human risk assessments, which is not in accord with current scientific thinking. Current EPA guidelines suggest that the use of quantitative risk assessment is inappropriate for weak oncogenic responses. Also, it is inaccurate to say 2,4-D and glyphosate are human carcinogens because the EPA has classified them into Group D, which means the evidence is inadequate and/or equivocal to classify them as being carcinogenic.

Data are available that have not been used in the creation of this document. The DEIS relies too heavily on brief, frequently outdated EPA data summaries that do not allow for independent review, data interpretation, and assessment for data quality and accuracy.

We have conducted a quantitative risk assessment for all herbicides with positive cancer studies or where credible scientific uncertainty exists regardless of the oncogenic response. We understand that the EPA does not conduct quantitative risk assessments in all cases; however, because of the scientific uncertainty associated with this class, we have chosen to conduct a worst case analysis of cancer risk to provide us with additional information to fulfill our responsibilities under NEPA. Also, we have added information on classification of the chemicals as carcinogens according to the EPA's weight of evidence system.

The EIS has been revised to include studies that have been validated by the EPA and to incorporate appropriate data based on information received during this review process. The risk assessment does not attempt to cover all the literature, but instead to focus on studies that show toxic effects at the lowest doses. We have revised the EIS to indicate that EPA data summaries were not solely relied on. Where these were our only sources of information, we confirmed the results with the EPA. In addition, University of Washington scientists reviewed surveys, key original studies, summaries, and literature.

Misleading conservatism and misuse of risk assessment methodologies run through this report. The very conservative quantitative risk assessment model used in this document greatly overestimates the cancer risks for humans. The exposure and health risks are based on several conservative assumptions that compound overestimates. Even though some scenarios are acknowledged to be highly unlikely in the appendix, the main document implies that the stated exposure levels are likely to occur.

The summary data may be misinterpreted by those who do not read the full document, because the conservative assumptions used are only set forth in the appendices.

Section D-3, page 7b: Bromacil NOEL should read "greater than 250 ppm (7.5 mg/kg/day HDT) rabbit teratology study."

Section D-4, page 27b: The half-life for foliar degradation of bromacil was excessive and resulted in greatly exaggerated exposure estimates (Table 4-9).

Section D-5, pages 28-29: The weight of evidence indicates that bromacil is nonmutagenic.

Section D, Attachment A, page 2: The authors incorrectly state that no validated mutagenicity data are available for bromacil.

This EIS is designed to err on the side of safety. We purposely used a conservative cancer model. We acknowledge that our exposure scenarios tend to overestimate risk. We clearly have laid out the assumptions that we used in the risk assessment. The names of the scenarios were defined to distinguish them from each other. We have added these qualifiers to the body of the EIS to better describe the risk.

We have revised the summary tables in the FEIS.

We have revised this sentence.

We used this half-life for bromacil to estimate risks as conservatively as possible.

We have revised this discussion in the FEIS.

We have incorporated this information into the FEIS.

Section IV, page 101: The diuron NOEL was based on the lowered red blood cell count (Table IV-19).

We have incorporated this information into the FEIS.

Section D-3, page 11: The diuron systemic NOEL should be 250 ppm (12.5 mg/kg/day).

We have incorporated this information into the FEIS.

Section D-5, page 16: The statement that backpack sprayers are "exposed to a diuron dose that exceeds the systemic NOEL is misleading. Therefore, the margins of safety reported for diuron and other products are correspondingly underestimated.

Doses to backpack sprayers are based on field studies for 2,4-D. The number of hours worked is also based on field data.

Section IV, pages 100 and 116: Code of Federal Regulations, Vol. 40, part 158, contains toxicology studies required for safety evaluations of products used for nonfood crop or forestry applications. Acute, subchronic, and special studies are required. The toxicity data bases for diuron and fosamine ammonium exceed these minimum requirements. This point also should be included with references to "missing information" for these products.

The required studies for nonfood crop use pesticides do not include all types of toxicity studies; teratology, reproduction, and oncogenicity studies are conditionally required. For the purpose of this analysis, more data regarding the teratogenic and oncogenic potential of diuron are needed to evaluate human health hazards.

Section D-3, page 22: There are no 2-year feeding studies with fosamine.

We have incorporated this information into the FEIS.

Section IV, page 117: References to toxic contaminants should be deleted because the EPA requires that the most extensive testing be conducted with technical grade or manufacturing use products.

We have revised this discussion.

Section D-1, page 1: Safety factors for inter- and intraspecies variability has been discussed with reference to hazard (margin-of-safety, MOS) assessments and extrapolations to man. These MOS values are actually larger when one considers the differences between the "threshold" and "no-effect" levels in an animal study (Fig. 301). This difference is variable, is related to the selection of doses at the beginning of the test, and can be equated with an additional MOS that is usually not quantitated. This fact should be considered when comparing MOS's for various products.

We have added this information to the discussion of MOS's.

Section D-1, pages 9 and 10: Costs for conducting toxicity tests are much higher than prices typically experienced by industrial or contract laboratories.

We believe that the EPA was disclosing experienced and typical costs for full product testing.

The discussion of possible risks from herbicides in smoke is a nonissue that should not be recognized in the DEIS, except to state that the matter is a peripheral concern for which there is no supporting evidence of a health hazard.

We do not agree that the discussion of possible risks from herbicide in smoke is a nonissue. We have clarified this issue in the FEIS.

Accidents attributable to wildfire control have nothing to do with vegetation management and therefore should not be associated with risk of accidental injuries from prescribed fire treatments.

We used the best available data to estimate risks of prescribed fire.

To produce desired risk results, the DEIS has manipulated data. In Table IV-16 on page IV-89, there is no consistent, proportional relationship between the realistic and worst case application rates for the various herbicides.

The referenced table was developed according to the following assumptions:

1. Realistic application rates are what the Forest Service normally uses.
2. Worst case application rates are the highest allowed by law.

Therefore, there is no linear relationship between the two scenarios.

Summary, page 20: In Figure S-6, risk of exposure to the public and risk of accidents to forest workers deceptively seem to be of the same relative magnitude. The risk to workers is actually much greater than the risk to the public.

We have revised this figure to better reflect relationships between worker and public risks.

Perceived health risk seems to be stressed heavily, even to the point of comparing between alternatives how much perceived risk the public will have. We believe the Forest Service can better change the perceived risk by educating the public rather than by selecting an alternative because it may have less perceived risk.

We have stressed the risk of potential adverse health effects rather than the perceived health risks.

The report never brings into perspective how infinitesimal the risk to the general public from herbicide exposure is, even in a worst case scenario.

In general, the exposure of the public is very low. However, based on laboratory studies and accepted scientific practices, under accident scenarios, the public may experience adverse effects.

Summary, page 18: The draft EIS implies that the public faces the same risk as a forest worker.

We have revised the tables to respond to your concerns. Please see the "Human Health" section in Chapter II.

It is difficult to critique the document because original references are not cited or are cited so poorly that they cannot be located.

Section D-3, page 9: I could not verify the EPA provisional ADI for 2,4-D of 0.01 mg/kg/day, but I point out that the World Health Organization established an ADI of 0.3 mg/kg/day for humans (1971 Evaluations of Some Pesticide Residues in Food, WHO Pesticide Residues Series No. 1, 1972).

Your 2,4-D information is quite incomplete and, as a result, inaccurate. On March 31, 1987, the EPA published a Lifetime Health Advisory of 70 parts per billion for 2,4-D in drinking water (0.07 mg/L/day). An official tolerance of 0.1 part per million in potable water was and is in operation.

Comparing the risks between alternatives by evaluating the number of acres treated (page II-25) is overly simplistic, as is comparing the environmental impact of the alternatives by evaluating the number of acres treated (page IV-22).

Living near areas aerially sprayed with 2,4-D poses a risk to human reproductive health in the form of a low-level hazard of birth defects and a threat of miscarriage throughout pregnancy.

We have tried to clarify references throughout the document.

The EPA's provisional ADI of 0.01 mg/kg/day was based on chronic studies that were not available at the time WHO set their acceptable daily intake. Therefore, we do not believe that the WHO reference is appropriate.

We have added the information on EPA's health advisory to the FEIS.

We have revised these tables to better characterize actual risks. In addition, we have prepared a separate summary on "Characterization and Management of Risk." Please refer to Chapters II and IV, "Human Health" sections.

Comparing the levels of exposure predicted for the public with the most conservative no-observable effect levels for reproductive and developmental toxicity indicates that there are large margins of safety. Negligible chances of birth defects or miscarriage would be expected in pregnant women exposed to the proposed herbicides.

Exposure to 2,4-D has caused peripheral neuropathy as well as many acute effects. Animal studies suggest a potential risk of cancer, supported by recent studies of human exposure.

Symptoms of 2,4-D drift have been noted on grapes 8 to 12 miles from the point of application. The chances of drift increase with wind and temperature. Persistence of 2,4-D in soil is about 1 month, in surface waters up to 6 months, and in bottom sediments up to 10 months. There is no information on 2,4-dichlorophenol, the first breakdown product of 2,4-D, which is more fetotoxic and carcinogenic than 2,4-D itself.

The worst case worker exposure for atrazine would be approximately 900 times less than the NOEL of 0.7 mg/kg/day derived from the chronic rat study, which is the most sensitive species based on the results of newly submitted long-term studies.

Subsequent interpretation of results is invalidated because the margins of safety were calculated as a ratio of chronic NOEL's and acute exposures, violating a general concept of risk assessment calling for a close match between the NOEL and the exposure scenario.

These potential effects are discussed in the EIS. Again, levels of exposure to the public under routine conditions do not indicate the potential for adverse health effects.

The Forest Service is proposing to use low volatile forms of 2,4-D, which do not drift as far as those used in the studies you cited. In response to your concern, we have added information on 2,4-dichlorophenol to the FEIS.

The NOEL of 0.375 mg/kg/day from the chronic dog study was established by the EPA based on increased heart and liver weights. Therefore, the dog was considered the most sensitive species, and this NOEL was used in the margin-of-safety determination.

You are correct that risk assessments generally match the NOEL and the duration of exposure. To be conservative, we have used a different approach and acknowledge that it tends to exaggerate risk.

Section D-3, page 13: The triclopyr fetotoxic NOEL listed as less than 10 mg/kg/day is shown in the original report and its subsequent publication to be greater than 25 mg/kg/day, which was the highest dose tested.

We have revised this section to respond to your comments.

Section IV, page 101: In Table IV-19, the triclopyr systemic NOEL is incorrectly stated as 0.5 mg/kg/day. However, Appendix D correctly states it as 2.5 mg/kg/day.

We have revised this section to respond to your comments.

Section D-1, page 9 and Table 1-2: The identification of "data gaps" is misleading. The table does not clearly point out what the x's indicate; also, the lack of a specific report to fill a mechanically derived slot does not mean that such data would be relevant for the material in question nor that the data have not been provided through other means (such as in another report that shows low toxicity or hazard and thus no need for such a test).

We have revised this table. In addition to our own judgment, we have shown what the EPA believes to be data gaps.

Although the DEIS notes that there are no neurotoxicity data for triclopyr, this is not the case because just about all of the mammalian tests on these compounds include careful evaluations of behavior during the study as well as gross and microscopic examinations of the central and peripheral nervous systems.

We agree with this comment and are including this information in our discussion.

Mattson et al. (Fund. & Appl. Toxicol., 6:175-181, 1986) found their results consistent with other studies that found neuropathologic consequences of repeated exposure to 2,4-D in rabbits, rats, pigs, chickens, or dogs.

We have revised this section to indicate that no neuropathologic consequences of exposure to 2,4-D have been observed in animal studies.

In Section 3, page 31, there is a statement concerning so-called evidence of synergism between picloram and 2,4-D arising from a skin sensitization test. There are many other possible reasons for the response, and synergism is only a remote one.

We have revised this section to indicate that the referenced study was one of many and that synergism is only one possible explanation.

New data, now available for triclopyr, show that the dermal penetration rate is 1.65 percent, with a standard deviation of 1.00 percent among human volunteers.

We have revised the estimated doses for triclopyr based on the new dermal absorption.

Section D-5, page 11: The assumption that all herbicide residue is released to the atmosphere upon burning is not correct. Data on many of these herbicides, including picloram and triclopyr, indicate that most of the herbicide is destroyed during burning and very little is released to the atmosphere even under nonideal conditions.

We have incorporated these data into the FEIS.

Is the assumption that "for both workers and the public, there is no convincing evidence that exposure to herbicides is more or less hazardous than the exposure to smoke" (page II-2) documented? This does not justify mixing comparisons of the two different types of exposures together. Prepare separate risk indices for smoke and herbicides.

We have revised the risk analysis to account for both components in smoke and computed a hazard index for the combination.

Although aerial applications of herbicides may present more risk to the public, that does not mean that ground applications are risk free.

Smoke from prescribed burns can travel long distances and settle into an air shed miles from the location of the burn. This is in contrast to the statement on page IV-119 that "dilution of smoke and herbicides is usually rapid."

What are the implications of the fact that the risks to the public from prescribed burning smoke are unknown?

Drift, runoff and ground-water contaminations also may occur from ground applications, so include them in the risk indices. Drift, runoff, and potential ground-water contaminations were considered in the risk from ground applications (see Appendix D).

We have revised this section and added appropriate references. However, the Forest Service avoids prescribed burning during stagnant air conditions.

We acknowledge the uncertainties you describe. We used a scientifically accepted method to estimate risks from the data that were available.

Comments and Responses to Public Comments on Human Health (University of Washington)

Comments

1. The document overstates exposures.
2. Herbicides are given greater scrutiny than other methods of vegetation management. They should all be subjected to a worst case analysis.
3. Analysis using workers' compensation records should be applied to all methods of vegetation management.
4. The analysis is too vague. There needs to be risk assessments done on a site specific basis.
5. The document states that there is not risk to workers from prescribed and wild fires--this is false.
6. The qualitative risk assessment is not sufficient. Qualitative risk assessments must be done and presented in the main document.

Responses

1. The worst case exposures are just that--worst case. They are not catastrophic worst case. They do not represent average exposures. They also do not represent some extreme cases of accidents or negligence. The Characterization and Management of Risk appendix discusses what is considered to be worst case.
2. Worst case risk assessment for herbicides was required by the courts as well as by a large section of the concerned public. Therefore, the analysis and EIS reflect that emphasis.
3. As explained in the Types of Risk section, workers' compensation records are not accurate for problems associated with repeated exposure, or problems that show up after time.
4. Site-specific applications are beyond the scope of this document.
5. We did not intend to convey that risks to workers from either prescribed fire or wildfire are negligible. These risks are now covered in Chapter IV and in the Characterization and Management of Risk appendix.
6. The qualitative risk assessment is revised in response to public and technical reviews. The background information is in Appendix H, and the analysis is covered in both Chapters II and IV under the Human Health sections.

7. The risk assessment needs to be more accessible to the general public. A comparison of the risks from vegetation management methods should be compared to everyday risks faced by the public.

8. There are too many assumptions. The risk analysis is biased, not scientific and not useful.

9. There is a big difference between risks chosen by people and risks forced on people. This needs to be stated in the document.

10. The exposures are not useful because they are based on assumptions; they are not accurate.

11a. The document should recognize the problems due to perceived risks. They are real. Discussing these risks enhances the actual risks. 11b. The document should not recognize the problems due to perceived risks. They are not real. Discussing these risks trivializes the actual risks.

12. Risks due to injuries should be directly compared to risks of cancer or other disease associated with herbicide and smoke exposure.

7. Comparison of risks is presented in the EIS. However, there is still a major difference between accepted risks and imposed risks. Personal habits such as smoking or diet can have major impacts on health and life expectancy. These practices do not necessarily justify imposing additional risks upon the public.

8. The risk assessment does require assumptions. We have tried to be clear and balanced and explain how risks were assessed in the Characterization and Management of Risk appendix.

9. We see the human health risks here as a by-product of managing competing and unwanted vegetation. As such, the risks are neither directly chosen by people nor forced upon them. Public involvement in selecting an alternative here (and in the development of projects later) determines acceptable risks and unacceptable risks.

10. Exposure assumptions are discussed. Average exposures as well as worst case exposures are considered.

11a & 11b. The DEIS acknowledges perceived risks. They are a real factor in the Forest Service goals of serving the public, as well as in its ability to effectively manage the forests. We do not use perceived risks in our analysis or estimates of effects. We rely on our detailed quantitative and qualitative risk assessments. Our experience is that perceived risks can not be easily dispelled by education. Many who feel strongly about these risks are very well educated and informed on the subject.

12. Comparison of actual data to estimated data can lead to erroneous conclusions. We chose to treat the two categories separately.

13. The Forest Service represents the risks to workers and to the public as being the same.

13. The risks to workers differ substantially from the risks to the public and this is pointed out many places in the EIS. The headings for some of the tables were not as clear in the DEIS as they could have been and they are changed in the Final EIS. The Characterization and Management of Risk appendix discusses public and worker risks separately and at length.

14. Wildfires have nothing to do with vegetation management. Thus, risks associated with wildfires should have no place in this document.

14. In the DEIS we had argued that the numbers of wildfires would be different between the alternatives. Public comments corrected us on this point and the FEIS does not include risks from wildfires.

15. The risks associated with herbicides and fire smoke are overestimated. The Forest Service is being too conservative.

15. The Forest Service intended to be conservative in the risk estimates. This is in line with NEPA direction and the purpose of this EIS.

16. There is no explanation of the "risk indices" for herbicides and fire smoke used in Chapter II.

16. The use of risk indices in the DEIS was confusing. The FEIS drops the use of indices and uses acres treated instead.

17. The document states that hand tools are more risky than herbicides for forest workers without proper documentation.

17. We did not intend to convey the impression that hand tools are more risky than herbicides for forest workers. The risks are different and difficult to compare. Misleading statements in the descriptions of the alternatives are corrected.

18. The EPA has registered these herbicides as safe. The Forest Service should accept the evaluation of the EPA.

18. The Forest Service is legally required to and must make its own judgements about risk. We need to assess how safe herbicides are for our uses, that is, in the context of how and why we are using them. EPA cannot do that for us.

19. The document ignores risks due to exhaust from equipment, noise, bee stings, poisonous plants, etc. There should be a full risk assessment for each of these risks.

19. The risk analysis concentrated on the major risks associated with each method of vegetation management. Assessment of all risks associated with working in the woods is really not helpful in selecting a vegetation management program.

20a. The data gaps are over-emphasized. The document will increase public concern beyond current levels and reason. 20b. The data gaps are under-emphasized. The document will increase public concern beyond current levels and reason.

21. The document confuses real and perceived risks.

22a. Various experiences with herbicides were related with harmful consequence. 22b. Various experiences with herbicides were related without harmful consequence.

23. The qualitative risk assessment is unscientific and should be left out.

24. The Forest Service use of pesticides is very small compared to other users. What is the big deal?

25. Dioxins were hardly mentioned in your discussion of inerts.

26. Your final evaluation ignores the problems of "inerts" and "interactions".

20a & b. Although the DEIS recognizes that there are major gaps in information on human health risks, we believe that the clear statement of what is and is not known will improve public confidence in the final EIS.

21. We have acknowledged perceived risks exist, but only analyzed known or estimated risks.

22a and b. Testimony about individual uneventful experiences with a chemical does not necessarily reflect upon its safety.

23. We believe that qualitative assessment of data upon which decisions are made is essential and that this is one of the strong points of this EIS.

24. The fact that pesticides are used to a larger extent privately or by other agencies does not relieve the Forest Service from the responsibility of considering the risks involved in its operations.

25. Dioxins are not considered inert ingredients, but are contaminants in some herbicide formulas.

26. We recognize the unknown parameters of herbicide use, including the inerts and interactions. However, while these are useful and considered in the overall evaluation of herbicide use, they cannot be used to compare between herbicides, because the full formulations are not available to us.

27. The human epidemiology is invalid because it included exposures to 2,4,5-T which contains more dangerous dioxins than 2,4-D.

28. The only contaminants mentioned in the herbicides were dioxins. Why were others ignored?

29. The document evaluates each herbicide and ignores the fact that there is so much missing data.

30. The document concluded that there were no significant problems for water, soil, wildlife and fish associated with the use of 16 herbicides without documentation.

31. The document is not scientific in its conclusions that some herbicides are carcinogenic.

27. The Human Epidemiology section looked at information on the class of herbicides which includes 2,4-D and 2,4-DP. This class also includes 2,4,5-T. It is rare to find pure and simple exposures in any working population. It was stated that the inclusion of exposures to 2,4,5-T was a problem for the interpretation of these studies. However, several of the studies had primary exposure to 2,4-D and still had similar findings.

28. Dioxins in 2,4-D and 2,4-DP were the major contaminants of concern. The literature reviewed had little information and expressed little concern about other contaminants.

29. The FEIS tries to fully recognize the missing data on herbicides. The quality of the data is specifically graphed for each herbicide for each type of toxicity. This data is to be used in conjunction with the quantitative data presented.

30. There are potential problems for water, soil, wildlife, and fish associated with the use of the herbicides. In the FEIS we have tried to cover these in more detail. The FEIS contains more information and discussion on fish and wildlife. Much of the material in the DEIS is now revised.

31. The DEIS did not conclude that any herbicide is a carcinogen. However, it did point out some evidence that would make it prudent to consider the possibility that some are carcinogens. The FEIS eliminates amitrol from all programs because it exceeded the threshold of acceptable risk for carcinogenicity. The analysis is found in the Characterization and Management of Risk appendix.

32. The document should take into account exposures to pesticides used by other agencies and private parties in the risk assessment.

33. The analysis shows that Alternative E, which is supposed to emphasize human health, has higher risks than some other alternative.

34. The risk assessment should consider the site specific use of each herbicide in its evaluations.

35. Health effects from herbicide exposure are not comparable to injuries during prescribed burns--why evaluate hazards from fire and herbicides together?

36. In Chapter IV, it states that "the public will seldom be exposed to herbicides...", but other analyses show that this is not true.

37. LAI concluded that 2,4-D was not a carcinogen, but the DEIS concluded that it was.

38. Health risk analysis should use information from recent court decisions.

32. Herbicide use in private and public practices off National Forest lands is not part of the Region's vegetation management program and so is not part of our analysis.

33. Alternative E was designed to reduce risks to workers and the public, but the analysis, which came later, shows that it actually resulted in some higher risks.

34. This is not a site specific document, so all herbicide use is evaluated for the general regional situation. Managers designing specific programs and projects will do site specific analyses using the policies and guidelines in this EIS.

35. We were trying to address similar kinds of risk (herbicides and smoke particulate) together. But we have separated them in the FEIS.

36. The Chapter IV statement is referring to exposures resulting from scenarios described. It was not intended to be a blanket statement. Worst case is different from what is normally expected on an average project.

37. In the DEIS we did not mean to draw firm conclusions about carcinogenicity, but talked about that information from a broader scope than that covered in Appendix D written by LAI.

38. Court decisions don't offer scientifically verifiable conclusions that could be used in our analysis.

39. Since there is insufficient evidence to prove that herbicides are harmful, there should be no restriction on their use.

40. Many of the risks evaluated occur in the future and thus are hard to evaluate.

41. There are faults with some of the epidemiology studies.

42. The discussion about dioxins in 2,4-D is too sketchy.

43. The analysis of human health risks does not account for immune impaired or sensitive people.

39. We are concerned about protecting human health in our programs. Lack of conclusive proof that a chemical is harmful does not demonstrate that it is safe. Our analyses are based on the best information available. They indicate that restrictions are needed for specific chemicals.

40. The DEIS discussed future risks and recognized that they are hard to evaluate. However, projections can be made from scientific studies and research.

41. No individual study is perfect nor does it determine, on its own, the toxic effects of a chemical. However, human data, despite the difficulties of multiple exposures and differing characteristics, are generally considered the best evidence for effects on humans. The human epidemiology data was taken from a number of different studies, and conclusions were drawn from the range of studies.

42. We believe the discussion meets the needs of this analysis.

43. The variability of individuals within a population is supposedly accounted for in safety factors applied to the data. However, hypersensitive people can react to levels so low that they are not covered by these safety factors. It is possible that some of the major health effects from herbicides would be due to hypersensitivity, and that these may occur more frequently than the predicted effects assuming a normal distribution of effects in the population. This is discussed more thoroughly in the Characterization and Management of Risk appendix, and are addressed in mitigation measures.

44. The Forest Service risk assessment does not account for mitigation efforts.

45. The DEIS does not distinguish between herbicides that are possibly carcinogenic and those that are probably not carcinogenic.

46. Anecdotal information should be used in evaluating herbicides.

47. The DEIS fails to prove the 16 herbicides are "safe" and therefore they should not be used.

48. The use of "risk indices" is not valid as risks are not proportional to the acres treated.

49. The worst case analysis of herbicides does not state the risks clearly enough.

44. Mitigation is treated separately from the risk assessment which is based on either current estimates of exposure or worst case scenarios. Decisions on policy and guidelines are based on whether mitigation measures can achieve an acceptable level of risk.

45. We now include a new appendix (Characterization and Management of Risk) and material in Chapter IV that discusses carcinogenicity and rates each herbicide according to the information available.

46. Anecdotal information does not usually lead to conclusions that can be verified for broad application. However, case reports in the scientific literature were considered.

47. None of the methods for managing competing and unwanted vegetation are in and of themselves safe. All pose risks. We try to identify the risks and decide how to deal with them in our program.

48. We thought the risk indices would help people compare alternatives. In the FEIS, we simply use acres treated and injury rates to compare human health effects of the alternatives.

49. In the FEIS, new material is included in Chapters II and IV and in a new appendix, Characterization and Management of Risk, in order to make the presentation on risk clearer.

Responses to Working Group (OFS, NCAP) Comments on Human Health (University of Washington)

Oregonians for Food and Shelter (OFS)

Comments

1. The assessments overestimate toxicity, the potential for exposure and the resulting risks of using herbicides. Since no risks are determined to be unacceptable, "perception of risk" is used to justify alternative methods. Accuracies and inconsistencies must be corrected.

2. The economic/sociological assessment in the DEIS doesn't acknowledge or evaluate real and traumatic health effects of decreased employment.

3. Alternative E is supposedly designed to "...improve the safety of forest workers when they apply herbicides and cut vegetation." Aerial herbicide application is far safer for workers than hand application because fewer workers are exposed to lower concentrations in a less direct manner for a shorter period of time. A 22 percent increase [of manual methods acreage]...is inconsistent with an alternative promoted as one to improve the safety of forest workers since manual vegetation is more dangerous work.

Responses

1. The worst case analysis calls for relating highest risk levels. The human epidemiology data brings into question whether this is in fact being done. In addition, perception of risk is not meant to be a substitute for the lack of information about real risk. It is an issue in itself for the Forest Service.

2. The effect of increased or decreased employment on public health is beyond the scope of this EIS.

3. Alternative E responded to one aspect of public concern. The DEIS did not mean to state that the substitution of ground application for aerial application would decrease worker risk. It would likely increase it.

4. Alternative B+ includes elements of seven alternatives in the DEIS. Paragraph on the bottom of page 25 [in the letter] provides the rationale for consideration of human health under this alternative--examine all techniques on equal basis, injury and toxicologic, choose lowest risk technique which meets design characteristics.

5. The Forest Service is to be highly commended [for commissioning peer reviews of the human health material in the DEIS.] The peer reviews "place much of the extremely technical information into better perspective, bringing to light the conservative assumptions made in the process." The reviews of Drs. Dost and Weir are noteworthy. Dr. Frank Dost's comments are very specific in pointing out technical deficiencies and errors. Dr. Weir's peer review focuses on the "problem of communicating the intricacies of risk to a skeptical public". Dr. Weir's comments specifically apply to DEIS authors' bias of an anti-herbicide nature.

6. Safety...is greatly understated in the DEIS in terms of reducing worker exposure and therefore reducing risk. Dr. Weir states..."The informed use of forest-worker training and appropriate personal protective equipment will minimize worker health risks from any herbicide application..." In 10+ years [a reforestation contractor] has had ZERO pesticide related injuries, sicknesses or other chemical malady. There were 75 non-chemical injuries during the same period.

7. For safety, there needs to be an objective and full assessment of all methods including transportation of crews to sites for manual methods. Accident records from these activities should be considered.

4. Alternative H, the preferred alternative, incorporates suggestions from many sources including "B-Plus", comments from the public review process, and peer reviews. Alternative H, we think, includes the best elements of all the input we received.

5. Some reviews believe that risks are overstated and some believe that risks are understated. We see these as differing professional opinions rather than bias.

6. While safe practices are essential, they are difficult to accomplish in reality. Safe practices are hard to establish in a shop with supervision, much less in the field. It would be incorrect to assume that "new" safe practices will substantially diminish exposure over current levels. In addition, the lack of reported problems is not indicative of the actual level of problems. Current reporting systems are incapable of picking up the vast majority of exposure related health problems.

7. The assessment addresses the major risks for each method, and only for activities directly involved in vegetation management. Many secondary risks, such as transportation to work would require too many detailed assumptions to be practical.

8. An in depth review is in order for worst case analysis of all alternative methods including evaluation of risk related to power and hand tools, sharp instruments, driving, exposure to fumes, etc.

9. What is the significant effect on health of herbicide contamination of surface water in forest environment? Reference page IV-46. Six year data say this isn't a problem. Why bring it up?

10. The Forest Service should not propose an alternative which bans the use of a registered pesticide or precludes use for its intended use as approved by the EPA.

11. The tone and language used in the DEIS is strongly biased against herbicides. The final draft should be more carefully written to eliminate such bias and protect against creating negative public perception itself--perpetuating perceived risk.

12. Pages II-25-28 in the DEIS. What do these risk indices mean in terms of increased cancers or accidents per million humans? ...we doubt that they will mean much to the decisionmaker.

13. References page II-37 and II-11. What do differences in suspended particulates mean in terms of human health? Where will the burning take place? If most of the burning will occur in areas devoid of population, will smoke really be of much health risk to humans?

8. We have added analysis of risks for all methods in Chapter IV. But the worst case analysis was required only for herbicides.

9. These levels were reported and considered in the DEIS. It is important to know what is not a problem as well as what is.

10. The array of alternatives was presented in response to public interest and comments. And although EPA registers herbicides for intended use, we must analyze their effects in context with where and how we use them.

11. Some reviewers thought we had "white washed" the effects of herbicides. But we tried to present the results of our analysis without bias in any direction.

12. Risk indices were intended to compare alternatives and not to give estimates of absolute risk.

13. Particulate measurements were taken in populated areas. Our mitigation measures are oriented toward effects on population centers and will achieve compliance with state standards.

14. Did the discussion on human health risks take into account the required mitigation measures? If not, what would be the estimate of human health risk considering the full use of the mitigation measures?

15. Page IV-122 - Paragraph 4--An indication is given of the upper estimate of risk of cancer from fire smoke. We believe that it would be beneficial to include a similar estimate for herbicides.

14. Mitigation measures and their effect on risk are discussed in the Characterization and Management of Risk appendix. They are fully considered in determination of policy on human health risk. We believe that the mitigation measures render the risks manageable and acceptable.

15. These are included in Appendix D. Their usefulness is questioned by evidence from human epidemiology.

Northwest Coalition for Alternatives to Pesticides (NCAP)

Comments

1. The implementation of Alternative B, with its equal ranking of chemical and nonchemical methods, will require a defense of the "safety" of herbicides; a defense the EIS has rendered untenable by its discussions of secret ingredients, inadequacy of toxicological data, and risk to the public and workers.

2. The policy of using herbicides as a last resort is reasonable. Most of the ingredients in an herbicide formulation are unknown to the Forest Service and the public and have not been tested. None of the revealed herbicide ingredients has been tested adequately to produce a risk assessment in which confidence can be placed. Herbicide use will not be precluded where nonchemical approaches are economically or otherwise infeasible.

Responses

1. There is certainly missing information about herbicides that makes it difficult to accurately characterize their risk. This is acknowledged and discussed in the human health material included in the EIS.

2. Alternative H, the new preferred alternative, provides for use of herbicides only when necessary, i.e., when other methods are not practical or are not economically feasible. See Chapter II.

3. The Final EIS's preferred alternative should eliminate 2,4-D, amitrole, diuron, and fosamine from being used in any application.
4. Extra worker safety considerations should be retained and the discrepancies in the EIS should be addressed. We suggest that you eliminate atrazine, bromacil, dicamba, simazine, triclopyr, and 2,4-DP from backpack spraying (including hack and squirt).
5. Aerial application of herbicides should not be categorically excluded. Decisions about the method of application should be made on a site specific basis. Worker exposure to herbicides from ground application is an overwhelming disadvantage that is not sufficiently addressed in the mitigation section of the DEIS. Workers using hack and squirt application techniques are at high risk. There are significant problems with aerial application. Prevention and nonchemical treatments are preferred.
3. Based on the risk analysis, amitrol, diuron, and fosamine are not permitted regardless of the alternative selected. The preferred alternative permits the use of 2,4-D when the conditions of the project analysis process are met. Additional mitigation measures are now included. See the Characterization and Management of Risk appendix.
4. Our analysis points out concerns for reproductive and developmental effects on workers for 2,4-D, triclopyr, atrazine, 2,4-DP, simazine, and bromacil. We will not use female workers in the application of these herbicides. Mitigation measures require protective clothing and equipment to reduce risks to an acceptable level. See the mitigation section of the Characterization and Management of Risk appendix.
5. The elimination of aerial spraying was only one approach offered in the range of alternatives in the DEIS. The alternative eliminating such spraying was designed to respond to concerns expressed by the public. Alternative H, the preferred alternative, does not eliminate aerial spraying. It does place special restrictions on backpack spraying of specific chemicals. In addition, we agree that mitigation, and especially worker training, are limited in their effectiveness. Nevertheless, they should be recognized as important elements in this program. We also recognize that there is very little information available about exposures to workers during hack-and-squirt operations. Your examples demonstrate that the potential for exposure is high, as in other ground spraying operations. Our analysis shows, however, that with appropriate restrictions an acceptable level of exposure will be achieved.

6. Prepare a profile of the major environmental impact concerns for each herbicide. Much of the information useful for these profiles is already present in one form or another in the DEIS, but it is scattered among text chapters and appendices.

7. Indicate the exact formulations the Forest Service intends to use (including the percent by volume of secret ingredients) and indicate the results of acute toxicity tests for each of these formulations. Provide background documentation that the acute toxicity tests have actually been performed ON THAT FORMULATION. When these acute toxicity tests have not been performed, clearly indicate that you therefore are missing even this minimal, limited information on the full formulations you will be spraying. Indicate that whether or not the secret ingredients and revealed ingredient(s) are in fact acting synergistically is simply not known.

8. Supplement the current discussion with new information of 2,4-D with new information. The Forest Service should entirely eliminate use of 2,4-D and 2,4-DP from Region 6.

9. Explain the significance and meaning of the data presented on mutagenic toxicity of the herbicides.

6. Summary profiles for the herbicides are now included in the Characterization and Management of Risk appendix.

7. Discussion of the lack of information on formulations of herbicides is included in the Characterization and Management of Risk appendix. Formulations known to contain only inerts on EPA Inerts Lists 3 and 4 may be used. All others are restricted from use. See the Mitigation section and Appendix J.

8. Conclusions of our analysis are that 2,4-D has demonstrated adverse effects including neuro-toxic effects and carcinogenic effects. There is some reasonable disagreement about reproductive effects. Unfortunately, the quality of data from Vietnam is so poor that we have found it difficult to include in the evaluation. Nevertheless, it does point to reproductive problems. Female workers are not to apply 2,4-D and 2,4-DP. See the Mitigation section in Chapter II.

9. Discussion in the Characterization and Management of Risk appendix describes how this information was used in the analysis and what it means.

10. Explain the basis for deciding when you have "moderate" or "high" confidence that NO adverse effects will occur to workers or the public.

11. The EIS must discuss the particular concerns of health effects from toxic chemicals faced by children, the elderly, and other sensitive people.

12. The EIS must discuss cumulative and synergistic effects of the revealed herbicide ingredients with the secret ingredients and the other toxic chemicals to which people are exposed.

10. The new Characterization and Management of Risk appendix describes how these ratings were calculated.

11. Some range of sensitivity is provided for in the confidence ranges. However, hypersensitive people or severely compromised people may fall outside this range. This is discussed more fully in the final EIS and appendices.

12. The Characterization and Management of Risk appendix discusses synergistic effects of active and inert ingredients. Exposure to other chemicals outside National Forest lands is beyond the scope of this EIS.

Issue: Public Participation

Comments

1. There was not enough time to respond to the draft EIS, the comment period should have been extended.
2. Many people did not hear of the planning effort until the last minute and had difficulty responding in time.
3. There is a need for greater public awareness and education about vegetation management in general.

Responses

1. We appreciate the time and energy people did invest in responding to the vegetation management draft EIS. NEPA regulations required only a 60-day review period. We decided on a longer, 90-day review, because of the size and scope of the document. A 30-day extension was granted in January, 1987, bringing the total review time to 120 days. We feel that was adequate time for interested individuals to respond.
2. We tried to use all possible avenues in advertising our planning efforts. During the comment period, local Forest Service offices sponsored meetings, mailings and issued news releases. People who responded to the DEIS are now on our mailing list. We recommend contacting the local Forest Service office with which you are most familiar to stay in future touch about vegetation management activities.
3. We agree. Our implementation strategy includes an important emphasis on working with people at all levels of our planning. In addition, specific training will be developed to better educate our own employees. Please see the Mitigation Measures and Implementation sections of Chapter II.

4. Public involvement is needed at the project and district level. Citizen committees, task forces should be established.

4. Involving people who are interested and affected by local vegetation management projects is a key part of the preferred alternative. It will be an integral part of the implementation plan. Local Forest Service offices will be responsible for developing techniques to work with people in their areas on vegetation management projects. Overall regional direction for public involvement related to Vegetation Management activities is found in the Implementation section of Chapter II.

5. How was public input used in developing the final EIS?

5. All the responses we received went through two levels of analysis. The first was to sort and catalog the thousands of comments into the seven public issues developed in our scoping process (see Chapter I in the FEIS for the seven issues). In the second phase, each ID team member synthesized and developed recommendations on how to respond to comments, or sets of like comments. The Comment/Response section of this Appendix is the result of that analysis.

6. How does this document reflect the goals, objectives and mission of the Forest Service?

6. This FEIS provides the environmental analysis necessary to implement a program of vegetation management activities in the Pacific Northwest Region under the NEPA process. Vegetation management is one set of tools we use as we manage resources to accomplish the mix of products and services outlined in our mission.

Issue: Social and Economic Effects

Comments

1. IMPLAN should be replaced with a model that really analyzes the demography and looks ahead to future trends such as decreasing jobs due to resource depletion and increasing transfer-payment revenues from retirees. Redefine model inputs to reflect this more realistic future, and then assess the alternatives based on these revised assumptions.
2. Chapter IV, Other Social Effects. Revise this section to realistically describe the reactions of these concerned citizens to the return of herbicides, including civil disobedience, media campaigns, and other forms of resistance.
3. "The assertions that jobs will be lost under Alternative D, for instance, is not substantiated in the text."
4. Economic and Social Effects would be positive under an IPM alternative because human and environmental effects would be placed at a premium without decreasing forest output.
5. Forest level impacts must be displayed.

Responses

1. IMPLAN could be modified with a great deal of effort to reflect the kinds of changes the comment suggests. We are looking at effects over the next ten years and do not expect sudden or sharp shifts in these areas over that time period. For more information on our reasons for staying with the IMPLAN model, please see Chapter IV, Social and Economic Effects, and Appendix B.
2. The narrative now includes further discussion of this comment in Chapter IV, the Social and Economic Effects section.
3. We clarified the rationale for job losses under Alternative D in Chapter II, Comparing the Social and Economic Dimensions of the Alternatives.
4. Since different individuals would likely have different perceptions about an IPM alternative, we have addressed that point in Chapter IV, Social and Economic Effects.
5. If allowable sale quantity effects are portrayed by Forest, then other socioeconomic data ought to be presented by Forest as well. We added tables for jobs, personal income, and payments to local governments by Forest in Chapter II, Comparing the Social and Economic Dimensions of the Alternatives section.

6. B-10 Use the Bureau of Labor Statistics model instead of IMPLAN. The BLS model is a much more realistic model.

7. If National Forest productivity is reduced, job losses throughout Oregon and Washington will be larger than you indicate. The Beuter report & data of Con Schallan of the PNW Forest Experiment Station show that these job losses occur not only in the timber industry itself, but in related fields & supply services, impacting not only rural towns but the major population centers such as Portland, Tacoma, and Seattle.

8. Why are the jobs coefficients for the Veg. Mgt. DEIS so different from those presented in the Spotted Owl DEIS?

9. B-23 Why are the personal income effects based on 1977 dollars inflated to 1987 dollars?

10. Economic benefits of manual release employment vs. aerial and costs and lack of employment have not been analyzed.

11. The EIS should provide explanation for the significance of the economic and social changes that are expected in southwest Oregon.

12. I question your figures in the change in jobs category. Did you consider those who will benefit from increased recreational use of NFS but who are just outside NF boundaries?

6. We explain why we retained use of the IMPLAN model over the Bureau of Labor Statistics model in Chapter IV, the Social and Economic Effects section.

7. We explain that we have considered the greater ramifications of our actions in terms of socioeconomic impacts. Indirect employment effects are considered in Chapter II, Comparing the Social and Economic Dimensions of the Alternatives and Chapter IV, Social and Economic Effects. You will now find footnotes in Chapter II.

8. The analyses deal with different aspects of resource management. Owls affected timber primarily. This EIS deals with timber, roadside vegetation, rangelands and all the other areas that vegetation management affects.

9. The IMPLAN model used to do the analysis is configured in 1977 dollars. Please see the discussion of IMPLAN in Appendix B.

10. Economic benefits of manual release vs. aerial costs and employment have been considered. We explain the rationale in Chapter IV, Social and Economic Benefits.

11. We added material describing effects on allowable sale quantity by Forest. See Chapter IV discussions of social and economic effects.

12. We discuss the extent vegetation management activities affecting off-Forest recreation in Chapter II, the Cost/Benefit section.

13. We feel that the "change of jobs" figures may be unrealistic. If a modified version of Alternative D was accepted, and labor intensive methods for vegetation management were employed, an increase in number of jobs would result. In addition, these would be jobs that would be available in the small logging communities that are being most hurt by the mechanization of the mills and the shipping of raw logs overseas.

14. I don't understand the difference between the loss of jobs (22,000 listed on page [11] of summary) and the loss of person days depicted on Chapter III-40 as varying from 11,950 to 13,910.

15. We need to know what jobs specifically will be affected by implementing any alternative.

16. The local economic impact model does not reflect the fact that productivity has increased.

17. All job statistics should be converted to Full-Time Equivalents (FTE).

18. Analyze the effects on local employment of manual release vs. aerial spray.

19. I am concerned about the economic impact that noxious weeds have on agriculture in Eastern Oregon.

20. Some Forests (i.e. the Siskiyou) are much harder hit than others.

13. Concerns over the change in jobs figures have been addressed in Chapter IV, the Social and Economic Effects section.

14. We clarified the text in the summary to give a more accurate picture of the actual jobs lost.

15. The document has been revised to show the number of jobs affected by each Forest. Please refer to Chapter IV, Social and Economic Effects.

16. The productivity level issue is addressed in Appendix B, in the section titled Uncertainty in the Analysis.

17. The implications of such a conversion are addressed in Chapter IV, the Social and Economic Effects section.

18. These considerations are included in the analysis in Chapter II, Comparing the Social and Economic Dimensions of the Alternatives.

19. Although the interference of noxious weeds in the National Forests have not been a major concern, we have expanded the narrative in Chapter IV, Social and Economic Effects, explaining our rationale.

20. Individual Forest information regarding such issues as long term sustained yield can be made available. The document portrays regional effects.

21. Consider the impacts on local and regional economies.

22. I found the interdisciplinary team's easy dismissal of the alternative that would have stressed the use of labor-intensive practices...disconcerting.

23. Like the negative figures for Alt. D presented in the DEIS, those for Alt. A are asserted without proof. The agency states for instance, that non-herbicidal timber management leads to an increase in hours required to do the job, but at the same time predicts a great loss of jobs if the non-chemical alternative is used.

24. The VMDEIS does not include individual National Forest impacts of the alternatives.

25. The VMDEIS does not include an adequate range of alternatives.

26. Predicted long-term sustained yield estimates for Alternative D are questionable and should be re-evaluated. (NCAP)

21. Since we have displayed additional information on allowable sale quantity effects, we chose to expand our portrayal of impacts on local and regional economies. We've also added tables in Chapter II, Comparing the Cost/Benefit Dimension of the Alternatives. The tables focus on jobs, personal income, and payments to local governments by Forest.

22. We added information on why the team dismissed the most labor-intensive alternative. Please see Chapter II, Alternatives Considered but Eliminated from Study.

23. We added clarification to distinguish between effects associated with changes in the harvest level from those effects not associated with harvest levels. Treatments are not traded off on a one-for-one basis. The relationship between allowable sale quantity and jobs is also emphasized. Please refer to Chapter II, Comparing the Social and Economic Dimensions of the Alternatives.

24. We added tables listing jobs, personal income, and payments to local governments by Forest. See Chapter II, Comparing the Social and Economic Dimensions of the Alternatives.

25. The statement calls for an alternative which promotes more positive social impacts in terms of employment and present net value than Alternative G. We have added discussion of another alternative that was considered but eliminated from detailed study. Please refer to Chapter II, Alternatives Considered but Eliminated from Detailed Study.

26. The Forest estimates of potential allowable sale quantity were used to test the Alternative D estimate. A consensus of opinion verified the long-term sustained yield estimate in the DEIS.

27. The Oregon Highway Dept. sees Alternative D as causing increased road maintenance and/or increased replacement costs due to structural damage.

28. Include alternatives that incorporate community stability alternatives generated for individual National Forests.

27. After further investigation we could not establish reliable information to indicate that increased maintenance and replacements costs would indeed occur under Alternative D. Our analysis remains the same.

28. This concern is addressed in Chapter II, Alternatives Considered But Eliminated.

Issue: Cost/Benefit Analysis

Comments

1. The impact of various alternatives on budget (cost) requirements appears to be inconsistent or uncorrelated in the DEIS.
2. In Table II-8, for Alternative G is $PNV\ 298 - 318 = -20$?
3. We urge that the one percent per year increasing real price trend be revised upward to approximate the actual trend and supply and demand interactions.
4. The Forest Service assumed an unlimited budget.
5. The DEIS should estimate and display the cost in benefits foregone since the injunction was invoked in terms of 1986 real dollars.
6. The "economic effects from changes in timber yields" you refer to here need further documentation and analysis in order to weigh them in decision making. The analysis needs more work in firming up the economic assumptions.
7. If you are going to mention the potential for tort claims because of lack of safe driving visibility, please discuss the potential for toxic tort claims from citizens and workers who are poisoned by the herbicides.
8. Revise the tables describing the jobs and personal income across alternatives after modifying the timber volume changes as discussed above.

Responses

1. We added information on costs included in the budget to the Cost/Benefit Analysis Section, Chapter II.
2. The comment is correct. The value shown should have been -20. This entire table is revised in the FEIS.
3. We address this issue in Chapter II, Cost/Benefit Section and to Appendix B. We discuss real price trends and what constitutes a credible range in this section.
4. We explain more fully budget constraints used in developing the alternatives in Chapter II, the Costs/Benefits Analysis Section.
5. A reference to cost in benefits foregone is made in Chapter II, the Cost/Benefit Analysis Section.
6. We clarify how allowable sale quantity effects are translated into economic effects in Chapter II.
7. We discuss the potential for toxic tort claims by citizens and workers in Chapter II, the Cost/Benefit section.
8. Allowable sale quantity figures are calculated and discussed in Chapter II. They are now broken down, Forest by Forest. Effects on jobs is included in this discussion.

9. DEIS Page IV-80--Do the costs for road maintenance shown in Table IV-15 include all costs for the 5-step management process delineated in Chapter II of the document?

10. Because the state administers resources whose economic values are not quantified as part of these evaluations, we are concerned about the consistently preferential treatment given timber and other commodities.

11. Recommend the use of the original Loomis and Sorg numbers for fish and wildlife and alternative values for losses of fish and wildlife.

12. Decisions to harvest "marginal" (for timber) lands and analysis of below cost sales should include the adverse effects (especially long term) of consequential vegetation management actions.

13. The estimates of changes in timber outputs and subsequent changes in county receipts and jobs are based on the change in long run sustained yield.

14. The overall economic impact table designating Economic Criteria Response to Changes in Allowable Sale Quantity Effects is incomprehensible.

15. Do the efficiency analysis for 150 rather than 100 years.

16. Charge that costs were not viewed as being a limiting factor.

9. Costs included in the 5-step management process have been included in the analysis in Appendix B.

10. The narrative in Chapter II, Cost/Benefit Analysis, illustrates how PNV is only one of many criteria considered during the analysis.

11. We explain in Chapter II why figures on fish and wildlife and recreation are not included in the analysis. They were not expected to vary significantly by alternative.

12. Certainly all effects, negative as well as positive should be included in a project analysis. As shown in Chapter II, the Cost/Benefit Analysis section, the various effects will be considered in making on-the-ground decisions.

13. We have clarified and expanded the text to better illustrate relationships between timber, employment, the regional economy, and sustained yield. The point discussed near the beginning of Chapter IV, Environmental Consequences, in the costs and benefits section, is "how much does vegetative management affect the entire picture?"

14. We agree that the table needed extensive modification. We modified the Sensitivity Analysis applying to the tables pertaining to lifetime doses.

15. This change incorporated. As we examined this in Chapter II, the Cost/Benefit section, we found the difference would be insignificant.

16. In Chapter II, the Cost/Benefits section, we more fully explain how we used the budget constraints in developing the alternatives.

17. Non-market values really have little place in this analysis. They are questionably developed by the Forest Service and are frequently called "funny money".

18. A 4% discount rate was used in forest planning in driving the FORPLAN model. The 7 1/8% analysis referred to is simply rediscounting a 4% solution. The analysis should have been done at the 7 1/8% rate to determine the sensitivity of the solution to differing discount rates. This is a flaw that needs correcting not only in the VM DEIS but in the forest plans as well.

19. The DEIS makes note that some costs were excluded from the analysis. What costs and why were they excluded?

20. Appendix B--The budget effects table is meaningless unless alternative B line items are identified.

21. We would tend to choose a discount factor of 5 or 10 but that decision is best left to professional forest managers.

22. It is highly unlikely that the Forest Service will find the adequately trained work force required for some of the manual and mechanical vegetation management programs. If a work force could be found, it would be much more expensive than the Forest Service estimates and the costs would rise further to pay for their training.

17. The values used in the analysis are market values rather than non-market values. No other values are included as benefits in the economic analysis because no other Forest outputs have been reckoned to vary significantly among the alternatives. This has been covered further in the Appendix B.

18. We added an explanation for why we used 4% in Chapter II, Cost/Benefit section.

19. We list in Appendix B the costs and benefits which are not expected to be affected by this decision and are not included. Examples are also cited.

20. Since allowable sale quantities and other Forest Plan data have been released, we used the actual numbers in place of Reference. That is shown on in Appendix B in the table on Direct Budget Effects Resulting from Vegetation Management.

21. No modification response is indicated here, however we thank you for your confidence.

22. These concerns have been addressed throughout the narrative.

23. My instincts tell me that a program which pays little attention to IPM, prevention, the potential impact to environmental, and instead attacks vegetation aggressively with herbicides without complete knowledge of possible side effects, will eventually be cost prohibitive.

24a. Since net public benefits have not been included in the analysis, it becomes impossible to weigh the significance of the net present value impact on the forests against public benefits. The difficulty of estimating public benefits is obvious, but those benefits that can be estimated should be included in the Draft EIS.

24b. For example, from an air quality, public health and visibility improvement standpoint, nationally accepted benefit values can be used to calculate a dollar based health benefit per ton of particulate reduction. Similar factors exist for visibility benefits and are available to the Forest Service.

25. Cost of various management regimes must be considered in full. Indirect costs of pesticide use include: a) This EIS b) Worker insurance c) Applicator licensing costs d) Staff time involved in notification; posting before and after spray operations and public meetings e) liability insurance f) cleanup fund g) staff time involved in chemical review h) nutrient replacement (fertilization costs).

26. Manpower and other costs for aerial application of herbicide are excessive.

23. We appreciate you sharing your views.

24a. We explain why there is no presentation of net public benefit included in the discussion of present net value. See Chapter IV and Appendix B.

24b. An explanation has also been included as to why a dollar-based health benefit per ton of particulate reduction figure is not included. This is included in Appendix B.

25. There are several important points made here. Many of the costs identified are already included (indirectly) in the analysis, such as liability insurance and workmen's compensation. Please refer to Appendix B.

26. We have checked the costs to examine their reasonability. We are comfortable they are representative of aerial application costs.

27. It is imperative that a timber demand analysis be undertaken.

28. Cost/Benefit ratios used are over simplistic and don't even consider all direct costs.

29. The forest plans assume that all vegetation is rendered free to grow after regeneration regardless of costs. The forests built into their average regeneration costs some expensive treatments such as hand release. However, the allocations and schedules are determined by using these high costs. We believe that the forest plans should have separated expensive treatments and traditional herbicide treatments and allowed the PNV objective function to work and thus be able to select the most efficient prescriptions for the plan. The VM DEIS simply incorporates whatever the forests did.

30. Indirect costs, such as crop tree damage, loss of beneficial species and unknown health damage are not considered.

31. FORPLAN estimates of timber values are too high.

27. Being mindful of the numerous analyses which have been conducted, we did not recognize a need to generate such an analysis for this particular study. However we have added additional information in the Cost/Benefit Chapter II.

28. We have included all the direct costs and benefits which we felt could be dollar-valued reasonably. This is included near the beginning of Chapter IV, the Costs and Benefits section.

29. We discuss why we relied on the Forest Plan effort for so much of our analysis. Please refer to Chapter IV, Environmental Consequences.

30. We acknowledge that for the regional analysis we incorporated these considerations in the economic analysis, however these indirect costs will be considered by the project-level decisionmaker in selecting an alternative. This is added in Chapter IV, the Costs and Benefits section.

31. The comment has been considered and addressed in Chapter IV, Environmental Consequences, the Costs and Benefits section.

32. Table II-9 estimates a very minor increase in annual budget for alternative A versus B. Does this take into account the past history of effectiveness of alternative control measures and the number of entries required to achieve yield goals?

33. It is difficult to believe the assumption that the Forest Service's budget would decrease if herbicides were not used.

34. The results of the sensitivity analysis and the uncertainty of economic assumptions and calculations identified in Appendix B must be acknowledged in the text.

35. You have included mitigation costs in the analysis. You see them as improvement work that should generate benefits.

36. These tables are incorrectly labeled for Alternatives A,C,D,E, and F with the benefit and cost labels reversed. Because the analysis is a marginal difference from the reference B alternative, the benefits presented in the tables are reduced benefits or costs. Likewise, the costs presented in the tables for the above alternatives are reduced costs of the program which should be labeled as a benefit of the alternative as compared to Alternative B.

32. We explain differences between harvest effects and pure vegetative management effects in Chapter II, Costs and Benefits.

33. We include clarification on the differences between direct vegetative management practices budget effects and allowable sale quantity effects. Please refer to Chapter II, the Cost/Benefit section.

34. References to sensitivity analysis and uncertainty of economic assumptions are elaborated on in Chapter II, the Cost/Benefit section, and in Chapter IV, Environmental Consequences.

35. Mitigation efforts are designed to eliminate potentially adverse effects of an action. Mitigation costs for vegetation management are affected by decisions concerning those projects. They have therefore been included in the analysis. The analysis also includes the costs of improvements which are also affected by decisions regarding vegetation management.

36. These tables are revised in the FEIS using actual figures instead of using comparisons to Alternative B.

37. Specific FORPLAN harvest scheduling should be done on each National Forest. The U.S. Forest Service should prepare such an analysis and provide an opportunity for public comment prior to issuing a FEIS.

38. I certainly suspect there are more impacts on recreation from herbicide use than temporary losses of berry picking opportunities.

39. Neither does the DEIS discuss commercial values of hardwoods and other vegetation classed as "unwanted" by the Forest Service.

40. The economic analysis does not anticipate that additional costly "corrections" will be necessitated by the disruption of ecological relationships through the artificial manipulation of vegetation. This is a reasonable expectation.

41. Describe the relevance of the data gaps concerning the 45% of commercial forest land with vegetation types where the need for herbicides are unknown.

42. We don't like your assumption of a horizontal demand function.

37. Generating the input data for a FORPLAN run and expressing it in a FORPLAN format would have been extremely time consuming. It would have provided little information that couldn't be gleaned from existing FORPLAN runs and other analysis. An additional section has been included on this in Appendix B.

38. We expanded the discussion on the impacts on recreation sites and recreation use in Chapter II.

39. We include an explanation of what is considered dollar-quantified vegetation and that vegetation which is not. This is discussed in Appendix B.

40. This concern is covered in the discussion of economic uncertainty in Appendix B.

41. In some cases there simply is not enough data available to accurately predict long term timber yield effects associated with the loss of herbicides. This discussion is found in Appendix A, Information and Research Needs section.

42. The kinds of changes in allowable sale quantity we are discussing are quite small. It would be extremely difficult to build a model that would accurately display useable figures for the small changes in allowable sale quantity anticipated. See more demand function information in Chapter II, Cost/Benefit, Comparing the Dimension of the Alternatives.

43. We could not replicate the cost/benefit analysis from the information given in the DEIS.

44. Treatment of Budgetary Constraints Needs Clarification

45. While you are considering the economics of herbicide use, please consider the long-term costs of your responsibility to cleaning up contaminants as well as compensations to people for adverse health conditions attributable to exposure.

46. Any definition of this concept ["last option"] should include cost-effectiveness as a strong determining factor.

47. Analyze the historical records of each National Forest regarding the assumptions made about the efficacy and costs of different treatments.

48. Longer rotations reduce the frequency of vegetative management actions and reduce the overall effect of competing vegetation over the whole rotation. While long rotations do not fair well in PNV analysis, they do in fact produce nearly as much timber, and of higher quality.

49. Should more clearly explain...[the] analysis and conclusions in terms of economics and economic efficiency.

43. We include a better explanation of how the cost/benefit analysis was produced in Chapter II, the Cost/Benefit section.

44. We include in the Cost/Benefit section of Chapter II, an explanation of how we used the budget constraints in developing the alternatives.

45. The concerns expressed are addressed in Chapter II, Comparing the Cost/ Benefit Dimension of the Alternatives. These costs, the costs of liability insurance and workman's compensation are included indirectly in the analysis. We have not explicitly considered any additional costs because we do not know that the costs would be incurred.

46. Last option is defined and the definition does include cost considerations. (Glossary and FEIS text).

47. We attempted to assemble our most accurate representation of what the efficacy and costs of different treatments are likely to be in the future. However we did not analyze Forest historical records regarding these costs since that would not necessarily be reflective of future conditions. Please see Chapter II, Comparing the Cost/Benefit Dimension of the Alternatives.

48. We added a section explaining that Forests may elect to reconsider certain aspects of their Forest Plans in light of this EIS. We acknowledge the accuracy of this statement. Please refer to Chapter I, in the Scope of the Program section.

49. We agree. The sum of all the changes in the process should address this concern. See Chapter IV and Appendix B.

50. Please implement the more rigorous standards of CFR 1502.22 in this discussion. Update the economic model and complete the analysis of existing data on vegetation management efficacy and related changes in tree growth so that realistic timber revenues can be calculated. Describe what you don't know and its consequences and then evaluate.

51. Your claims about being able to make a clear choice, based on timber yield tables, real price increases are misleading because of the lack of ground-truthing and other concerns.

52. Although the DEIS admits that the future effectiveness "will be near current levels" and that the costs are similar, the benefits obtained are all less. This discrepancy does not make sense and therefore needs serious analysis and discussion in the final EIS.

53. The Forest Service nowhere considers the claimed offsetting savings, realized from the use of herbicides, in its consideration of the expense of performing necessary research. Rather than relying upon the single-dimension cost analysis of Labat-Anderson, the Forest Service should have sought the viewpoints of its own economists, who surely would acknowledge that the Forest Service realizes both costs and benefits from the use of herbicides.

50. Additional data and literature was reviewed and, where appropriate, incorporated into the timber growth and yield analysis. These potential changes in timber yields were used in the development of the economic model and projection of timber revenues under each alternative. This information is found in Appendices A and B.

51. A response concerning "ground-truthing" real price increases is included in the narrative in Chapter II, the Cost/Benefit section and Appendix B, the Economic Efficiency Analysis section.

52. The question of potential effects of vegetation management on timber harvest levels has been revisited in the development of the FEIS. The revised information is found in Appendix A.

53. We added a discussion on potential savings available from using herbicides. We acknowledge the benefits of using herbicides and potential savings available.

54. Relook at the economic assumptions in your simulation model. New information on fossil-fuel price increases, slowed GNP growth, increased expense for personal injury lawsuits and increased mitigation costs should be added to the cost/benefit model. The model should also reflect future revenue losses due to environmental degradation from repeated intensive industry-forestry projects on the private and public lands as a whole.

55. The VM team should have thoroughly reviewed the analysis and assumptions contained and used in each draft forest plan instead of utilizing some of the forest data Carte Blanche (sic).

56. The forest plans used stumpage values and costs which are years old, are incompatible with each other and bear little relationships to values and costs today. The vegetation management team simply used the forests costs and values, and unless verified by the Forest Service, we cannot find credibility with the present net value calculations in the DEIS.

57. The costs and values used in FORPLAN influence the total amount of land allocated to timber management. In areas where assumed costs are high and stumpage values relatively low, lands are kicked out of solution as not being "cost-efficient." Of course this has a direct impact on LTSY as there are fewer acres and less intense management in the solution.

54. We have attempted to illustrate how various sensitivity tests can show net effects of such changes by changing the value of timber benefits or timber costs. For example, if changes, i.e., those listed in the comment cause increases in logging costs or silvicultural costs, those changes would be displayed in either lower stumpage values or increased Forest Service costs. Please refer to Chapter II, Comparing the Cost/Benefit Dimension of the Alternatives.

55. The economic analysis by the vegetation management team did rely heavily on Forest Plan work. We have explained this in Chapter IV, the Cost/Benefit section

56. Costs used in FORPLAN models were based on locally experienced costs. Prices were based on a run of years that included both highs and lows in the business cycle. Further explanation may be found in Chapter IV, the costs and benefits section.

57. Many Forests have substantially no land falling out of solution due to economics. Further, we asked Forests to assess the effect of each alternative on potential yield under current plans and allowable sale quantity under new plans. The results are displayed in two tables in the Timber Section of Chapter IV.

58. The DEIS does not address the impacts on the tourist industry from vegetation management, and does not consider the impacts on society from illness due to herbicide and smoke. "I will not visit areas where herbicides are used along roadsides or where aerial applications are planned, and I will not visit the forests during slash burning episodes."

59. Specific Forest Plans mentioned some uses of herbicides were anticipated, didn't mention others. The analysis relates only to forage and timber.

60. We want more details on budget effects.

61. Page IV-57 states that the FEIS may have implications for allowable sale quantities or long-term sustained yields. This statement suggests there are impacts not presented in the DEIS. What are these implications? How will they impact ASQ and LTSY?

58. We have attempted to clarify questions regarding social concerns. Please refer to Chapter IV, Social and Economic Effects. We have also responded to the issue of impacts on the tourist industry in Chapter II, Comparing the Cost/Benefit Dimension of the Alternatives.

59. All of the Forest Plans assumed that herbicides would be available. Language was not intended to be exhaustive or exclusive. Our analysis tracked benefits associated with timber production and livestock grazing, the two areas in which we could identify dollar-quantified benefits. The analysis also included other program areas as indicated in the document. Please refer to Chapter I, Vegetation Management and Forest Plans. Information is also included in Chapter II, Comparing the Cost/Benefit Dimension of the Alternatives.

60. We revised appropriate sections to distinguish between allowable sale quantity impacts from pure vegetation treatment effects. We modified the table called Estimated Change in Annual Forest Service Budgets to show budget changes resulting from allowable sale quantity effects for each alternative. See Chapter II, Cost/Benefit Dimension of the Alternatives. The Direct Budget Effects table and the section on Budget Considerations are also modified.

61. The effects of each of the vegetation management alternatives on long-term sustained yield (LTSY) were shown in Chapter IV and Appendix A of the DEIS. We have added expected effects on allowable sale quantity in the final EIS in Appendix A.

62. Herbicide administrative and supervisory costs as delineated in the DEIS are unrealistically high for operations done on a large scale.

62. We provide more background on our cost derivation process in the FEIS. Not all herbicide operations are of large scale. Please see the Description of Methods section in Appendix E.

Issue: Effectiveness of Techniques

Comments

1. The longer duration of treatment effectiveness of herbicides in some situations should be stressed more strongly.
2. The value of grazing for release should be presented in a "realistic" context.
3. Some key-associated species and herbaceous competition should be more clearly identified in Appendix A.
4. Use the ORGANON yield model instead of DFSIM.
5. Define the biological growth goals used in measuring timber yield effects.

Responses

1. The consideration of treatment effectiveness periods, and site re-occupancy by competitors is presented in Chapter II (Methods), Chapter IV (Yield Effects), and Appendix E (Method Effectiveness). Duration of treatment is a major variable in Appendix A (Growth Yield Analysis).
2. The description of operational considerations is slightly expanded. The use of livestock in conifer release programs is presented in Chapter II (Methods) and Appendix E (Program Effects, Method Effectiveness, and Substitutability).
3. This suggestion is beneficial in characterizing the yield assessment methodology. Descriptions of associated vegetation, specifically sprouting shrubs and herbaceous vegetation, are discussed in greater detail in Appendix A.
4. The rationale behind the use of DFSIM rather than ORGANON is explained in Appendix A (Attachment #13).
5. The basis of timber yield objectives is re-emphasized for clarity. The fact that managed yield tables developed in Forest Land Management Plans represent timber yield objectives is described in Chapter IV (Yield Effects), Appendix A (Growth and Yields), and Appendix E (Silviculture Program).

6. Studies related to growth and yield which were identified in the public response period should be incorporated in the FEIS.

7. The reforestation backlog and need for replanting should be addressed more fully.

8. Link the yield analysis methodology to some established science or study.

9. There is no evidence that restrictions on the use of prescribed fire will indeed increase the risk and severity of wildfires.

10. What is meant by the first clear sign of damage?

11. Nothing was said about grasses as target species.

6. We agree that some pertinent studies should be recognized and included in the document. Several new citations are described in Appendix A. They are in the Timber Growth and Yield Effects section.

7. Yes, this serves to clarify timber yield estimates. The discussion is expanded in Appendix A. Assumptions and several of the vegetation analyses are also addressed in the Timber Yield section.

8. The suggestion is helpful in establishing reliability of the estimates. We added material to Appendix A, Process Steps and Methodology.

9. We agree. Wildfire incidence and severity have been eliminated as variables among alternatives. The Fire sections in Chapters III and IV have been rewritten to eliminate speculation about the effects of vegetation management alternatives on losses to wildfire. The Fire section in Chapter IV presents the case for this decision. The effect of wildfire in the economic and human health analysis was held constant among the vegetation management alternatives, using the average annual burned acreage projected in the preferred alternative of each forest plan as the loss figure.

10. That language is not part of Alternative H. Damage is related to growth loss or spread of noxious weeds that prevents achieving forest goals. First clear sign of damage includes things such as stunted growth of planted trees due to competition from brush or seedling branches dying back from lack of moisture.

11. Please refer to the Chapter III section on Vegetation for discussion of grasses as target species.

12. Herbicide use allows spread of noxious weeds by opening habitat.

13. Include in the discussions the ineffectiveness of herbicides.

14. We should be getting more money for the less effective methods, monitoring costs, if Alternative D and untested prevention techniques are used.

15. Prevention methods are not proven, therefore dependence on this approach will result in significantly reduced outputs and increased costs.

12. The residual effects of herbicides do not generally cause immediate colonization of weeds on a roadside that has been treated. The most susceptible areas are treated repeatedly, particularly when noxious weeds are involved. The probability of stimulating the spread is extremely low, and the tradeoffs for treatment are usually well worth the low risk. See the noxious weeds section in Chapter III.

13. The disadvantages of herbicides including ineffectiveness are discussed in Chapter II, the Methods and Mitigation section.

14. We will cover the cost of completing targets including monitoring costs as required by law and Forest policy. A section in Appendix B is included to cover this concern.

15. Although prevention methods have not been proven, we cannot assume that they are necessarily more expensive. Prevention measures have been shown to be efficient and economical.

Issue: Environmental Effects

General

Comments

1. The analysis of particulate emissions needs to be done for PM-10 and TSP (Total Suspended Particulates) as well as for PM-2.5. The PM-10 standards promulgated July 1, 1987 (52 FR 24634) must be addressed.
2. Riparian is the best path for noxious weeds.
3. The DEIS underestimates the air quality and health effects of prescribed burning at locations immediately downwind of fires.

Responses

1. No additional analysis was needed in this case. Essentially all (99.99 percent) of smoke particulates are smaller than one micron. Therefore, an analysis of one standard stands for all three. Information has been added on (Particulate Matter) PM-10 standards to Chapter III, Air. This provides our reasoning for information on PM-2.5, rather than PM-10 and TSP in Chapter IV, Fire.
2. Noxious weeds are spread in a number of ways. Dispersion of seeds by streams is one. The methods of controlling noxious weeds in riparian zones must be compatible with management objectives for those zones, i.e., water quality may not be compromised. See noxious weed and riparian and fisheries sections, Chapters III and IV.
3. Health effects of smoke are accelerated by extended exposure to higher smoke concentrations. Standards by which the smoke management efforts are judged are contained in the state implementation plans. Language has been added to Alternative H elevating the health and safety concern for rural residential and recreating populations. Qualifying text has also been added to Chapter IV, the Soil Resources section.

4. The conclusion is inappropriate that visibility impacts from prescribed burning in eastern Washington and Oregon are not an issue.

5. Depending on the alternative selected as a reference, the smoke reduction figures can be made to look better than they will actually be.

6. The discussion on possible effects of prescribed burning on climate is incomplete. The release of carbon dioxide into the atmosphere when vegetation is burned is contributing to the greenhouse effect, causing global and regional warming.

7. Monitoring is not in the prescribed burning mitigating measures--it should be added.

8. Diversity should be more completely handled.

4. We agree. The fact that total area-wide emissions will decrease under all alternatives does not insure that visibility cannot adversely affected. The narrative has been revised to acknowledge there is a potential danger here and that extreme care must be taken to preclude wilderness visibility degradation. Please refer to Chapter IV, the Fire section.

5. The reference used is independent of any alternative. It is the amount of smoke produced in a year representative of the period 1985 to 1987. The narrative has been expanded in Chapter IV, explaining the basis of reference for smoke reduction claims. We've also clarified related information in Chapter II, Comparing the Environmental Effects Dimension of the Alternatives, and in the figure in the Summary.

6. The burning of vegetation and debris that will increase the vitality and capacity of ecosystems to produce biomass will actually produce a net decrease in atmospheric carbon dioxide. Statements have been added in Chapter IV, the Climate section. We have elaborated on positive forestry practices as they relate to land clearing, burning of fossil fuels, the carbon dioxide cycle, and photosynthesis.

7. We agree. We have developed monitoring guidelines and added them to Chapter II, Prescribed Burning Methods.

8. Diversity needs to be handled fully at the Forest level. We have expressed the requirement and policy to maintain diversity.

9. We doubt the long-term benefits of vegetation management.

10. Control upsets the vital plant/plant relationship.

11. Unwanted species are a part of the natural ecosystem.

12. The definition of IPM lacks essential elements we think are important.

13. List all associated vegetation by habitat.

14. We need to be monitoring both implementation and results.

15. List T & E by habitat and non-target.

9. Appendix A, Timber Growth and Yield Analysis, covers the benefits of associated vegetation and discloses the lack of linkage between young stand growth and stand growth models.

10. In most cases, noxious weed control is an attempt to restore the balance that existed before the invasion of exotic weeds. In other activities, reducing the relative cover is not considered significant since all relative relationships are also found in nature. Elimination of the competitor is not the goal.

11. This subject is addressed in Chapter III, Vegetation. There has been a minor revision in the wording of how associated vegetation is a matter of concern.

12. IPM means different things to different people. Our purpose is not to sanction any one view, but to operate with the basic principles. See Chapter II, the EIS and IPM.

13. We believe that listing all vegetation by habitat would not add to the conceptual framework of the EIS. References are available for more Forest- and habitat-specific species lists.

14. The FEIS acknowledges that we need to monitor both how we conduct our projects as well as the results of those projects.

15. We are unable to do this because there are over 400 sensitive species and tens of thousands of nontarget species in Region 6. Each Forest has listed sensitive and threatened and endangered species by habitat. In addition, there are partial lists of other vascular plants.

16. We should measure effectiveness against total biological potential.

16. We needed clarification on what the total biological potential is. We use conifer biomass as a measure of fiber production potential. However this falls short of biopotential if we include species and products other than conifers.

17. We need to have a diversity index.

17. A major project, in terms of time and cost of production, a diversity index is outside the scope of the EIS project. Forest Plans will provide specific information.

18. Concerns: diversity, ecosystem health.

18. We are certainly concerned about diversity and ecosystem health as well. Due to the general nature of this comment, we made no additions to the narrative regarding the comment.

19. Monoculture is not as productive as mixture.

19. This is true, especially if we define production in terms of conifer biomass. Biomass production can be enhanced by mixing species.

20. Riparian zones provide effective paths for the spread of noxious weeds.

20. In order to avoid violating buffer zones in riparian areas, water quality must take priority and may limit control measures in riparian areas. See mitigation measures in Chapter II, and discussions of noxious weeds and riparian resources in Chapters III and IV.

Fire and Air

Comments

1. Possible benefits of logging residues are not considered.

Responses

1. We added information on residue effects. Areas involved include undesirable vegetation, soils, damaging animals and insects, beneficial animals and insects, site productivity, and reforestation including planting costs and seedling survival.

2. There is no evidence that restrictions on the use of prescribed fire to reduce fire hazard will indeed increase wildfire losses.

3. There is no documentation of the process followed to develop the particulate emissions projections.

2. That is true. No such claims should imply that in the EIS. Decisions about prescribed fire to reduce fire hazard are outside the scope of this document. We are projecting no change to wildfire losses based on the assumption that any increases in losses will be offset by reduced losses from escaped prescribed fires. See revisions in Chapters III and IV, fire sections.

3. We added Appendix K to document the process we used in developing particulate emission projections.

Threatened, Endangered and Sensitive Species

Comments

1. Habitat alterations adversely affect threatened and endangered plants and animals.

2. Undesirable plants will become more competitive than threatened and endangered plants regardless of our actions.

3. The document is not clear that Section 7 of the Endangered Species Act has been complied with.

4. Site-specific biological evaluations will be completed prior to any treatment.

Responses

1. These would be identified in the site-specific biological evaluations required by the alternatives. See chapter II discussions of the alternatives.

2. This is outside the scope of our project and research.

3. We inserted an explanation about cooperation with the U.S. Fish and Wildlife Service to better emphasize that we are complying with regulations in Section 7 of the Endangered Species Act. See Chapter IV.

4. We added material to the section on completing site-specific biological evaluations prior to any treatment. See Chapter II.

Wildlife

Comments

1a. Inerts are unknown. 1b. Inerts is a misnomer and shouldn't be termed as such; [they] are secret ingredients in the formulations. 1c. Synergism with other chemicals is not known. 1d. Inerts are more toxic to wildlife. 1e. Inerts are not discussed/disclaimed, just dismissed.

2. Need far more detailed and quantified effects analysis on wildlife similar to that in Forest Plans.

Responses

1a-e. We added a discussion of "inerts" as related to wildlife species. Detail on this is found in the revised wildlife risk section of Appendix J.

2. The emphasis of the regional EIS is to encourage detailed site-specific analysis at the project level. This includes the use of analytical tools developed in the Forest plans.

Range and Noxious Weeds

Comments

1. The document is biased toward West side interests, and not the East side which has the burden of livestock management.[2] There is no mention of indirect benefits of...range management projects to overall resource, erosion control, nutrient cycling, water development and nitrogen fixation. [3] There are differences between sheep vs cattle grazing in vegetation management, and [4] grazing is not a biological control; it is a method of a physical control. [5] Livestock are the cause of noxious weed problems and destroyers of riparian zones, and [6] past management practices have not protected areas from livestock damage and overgrazing.

Responses

1. Revisions of the Range Ecosystems and Noxious Weeds sections in Chapter III, and refinements in Chapter II, Biological Methods, address such concerns as Westside bias and the effects of livestock grazing. We also clarify the difference between grazing and biological control methods. See Appendix F for refinements made in the section, Rangelands of the Pacific Northwest.

Soil, Water, and Fisheries

Comments

1. Need more analysis of potential for groundwater contamination-rate of herbicides for leaching potential.

2. Need quantification of effects on fish for each Alternative, i.e. sediment and herbicides. Relate to loss of fish numbers.

Responses

1. The Chapter IV table Comparison of Herbicides shows a number of ratings for various herbicides. We've considered a hazard rating for groundwater contamination and surface water transport. However, this depends on so many other factors besides the chemical. These include soil type, slope water table depth, and stream flow. Information from the E.P.A. National Pesticide Survey list of leachers (nine herbicides on our list) is included in the Comparison of Herbicides table and the Chapter IV, Water discussion. The table in Chapter IV, Comparison of Herbicides has been revised. Also, "mobility" refers to surface water transport, while "persistence" refers to soil.

2. A number of modifications were made as a result of this comment. Documentation is modified in Chapter IV, Riparian Resources and Fisheries. We have stressed prevention of impacts with mitigation measures. Quantification is not recommended. Discussion is expanded on the impact of an accidental spill in a pond or stream in Appendix D, section four. Please see Chapter IV, Riparian Resources for consideration of lakes, ponds, and wetlands. Also addressed are implications for fry, juveniles, and sensitivity as it relates to timing. A corrected discussion on bioconcentration factors in fish is also included.

3. Mention benefit of some species on soil productivity (alder, ceanothus fix N).

4. Note that most (90% or more) of the herbicides used will be intercepted by foliage and thus will not reach the soil until it is mostly decomposed. This statement is maddening! Where does it go? Is there that much foliage in new clearcuts or rangelands?

5. Discuss the ability of the Forest Service to monitor for the herbicides. It is important not to use a chemical you can't monitor for (glyphosate).

6. Table IV-9, (Recommended Concentration Maximums for Silvicultural Chemicals by Stream Class and User Group) was not adopted by the EPA for drinking water standards.

7. Improve, clarify discussion of environment, fate of herbicides in water.

3. We elaborate further on this topic in Chapter IV, Soil Resources. We have added ceanothus as a nitrogen (N) fixer, and clarified the impact as it relates to long-term soil productivity.

4. We agree and have acted upon this. Your comment is valid and the statement is deleted from the FEIS. Please refer to Chapter IV, Soil Resources.

5. We require that monitoring be done. Please see Chapter II, Herbicide Mitigation Measures. It is also addressed in Chapter IV, Water Resources. We have monitored for glyphosate in the past. Also covered in the text are the use of tracer dyes and spray cards. A discussion of uncertainty in accuracy of lab testing for picloram and 2,4-D is included.

6. This table has been deleted.

7. Several steps were taken to address this comment. We relate the discussion on drift in mountainous regions (cold air flow) in Chapter IV, Water Resources to Appendix D, section four. We have integrated material in the Soil and Water sections with other sections. Better assessment has been provided for ephemeral stream channels and timing in Chapter IV, Water Resources. Some clarification revisions are included on ratings listed in Chapter IV, the Comparison of Herbicides table.

8. Define "downstream" water user and "adjacent" landowner, what the notification procedure is, and whose responsibility (DOT's). It is important to notify all in the region so they can cover gardens, move stock, etc., and be prepared for the side effects.

9. Monitoring reports should be submitted to the Regional Forester and Oregon Department of Forestry/[Washington] DNR within 60 days after completion of the project.

10. Add water quality monitoring for downstream contamination from livestock grazing. Concern about bio-contamination from livestock (giardia).

11. Livestock should be excluded from riparian areas; what is strict control? [Chapter II, Biological Mitigation Measures.]

12. Add mitigation measure for burning to protect unmerch [unmerchantable] vegetation in riparian areas.

13. Require use of portable sanitation facilities for all personnel working in municipal watersheds.

8. A "downstream user" is defined as anyone located within two hours of stream travel time or within 2 miles. "Adjacent" means located within one mile. This has been related to width and transport. We have elaborated on the 15-day notification procedure that relates directly to landowners. Please see Chapter II, Herbicide Mitigation Procedures.

9. A 60-day report would not apply to all projects, but monitoring reports will be submitted to state agencies when required.

10. Livestock operations used for vegetation management will follow existing direction for protection of resources. Wildlife species are often the most important carriers of giardia. See Biological Mitigation Measures, Chapter II.

11. It is not practicable nor possible to eliminate livestock from all riparian areas. We added an explanation of limitations of the program and clarified restrictions on consumptive use in sensitive areas, such as those located immediately above campground springs. Please refer to Chapter II, Biological Mitigation Measures, and Chapter IV, Water Resources.

12. Specific directions for protecting riparian areas and threatened, endangered, and sensitive plants are found in regional guides and in individual forest plans. See Chapter II, Prescribed Burning Mitigation Measures.

13. Such facilities are standard requirements for all proposed projects. Chapter II, Mitigation Measures Common to All Methods and All Alternatives, outlines scoping and EA procedures. Guidelines for municipal watersheds are contained in Forest Service Manual 2500.

14. Spill management equipment should be carried on batch vehicles. It's safer to haul concentrates in sealed drums and mix on site. a) Department of Transportation (DOT) does not want to use a pilot vehicle when driving their tanker--define size of tanker. b) Is a spill plan required? Even for DOT? Who prepares it?

15. What about disposal of excess herbicides? Where do they go?

16. Add all applicable State laws, rules, and regulations, including the OR [Oregon] Forest Practices Rules.

17. On Fig. II-3, why weren't soil disturbance, water quality, fish and wildlife habitat included under Environ. Effects? Biomass production is not a very good unit of measure for environ. effects.

18. Improve the discussion of fish-riparian in Alt Comparison; not very informative.

14. Chapter II, Herbicide Mitigation Measures, now outlines specific requirements of the Department of Transportation in spill management coordination and responsibility.

15. Forest Service Handbook 2109.12 establishes complete instructions for all phases of herbicide use management, including disposal of excess chemicals. Chapter II, Herbicide Mitigation Measures, outlines this program. Management of herbicides will be further discussed in the operations and monitoring plan implementing the vegetation management program.

16. All state and federal laws, including the labelling instructions of the Environmental Protection Agency, will be strictly followed. See Item 2, Chapter II, Herbicide Mitigation Measures.

17. The items were omitted from the table on How Alternatives Respond to the Issues because there was no significant variation by alternative. This was due to buffer strips and other restrictions.

18. Information on the effects on riparian areas, fisheries, and threatened and endangered species is revised to clarify effects of sediment on fisheries.

19. Identify data gaps in environmental wildlife and fisheries impacts under 1502.22 NEPA. a) Missing info on interactions among different species and between different species and their environment that provide stability to specific ecosystems. b) Role of surfactants, solvents, etc., and their degradation products in terms of cumulative and synergistic effects is unknown. Toxic compounds, metabolites, secret ingredients, etc., unknown.

20. It is significant that you didn't mention the Clean Water Act, Safe Drinking Water Act, Migratory Bird Treaty or Endangered Species Act in other legislation affecting this EIS.

21. Mention beneficial uses of NF water, such as private domestic use, industrial water supply, livestock watering, commercial navigation and transportation, anadromous fish passage, spawning and rearing.

22. It is unclear how any of the mitigation measures deal with protection of soil and litter decomposers, except in riparian areas.

23. The analyses only discuss effects of the active ingredients--not formulations. There may be additive and/or synergistic effects from "inerts" in the formulations.

24. Encourage water quality monitoring after first major storm within 30 days of application--sample on rising hydrograph. Experience has shown this is when highest concentrations occur.

19. We included information on data gaps in regards to wildlife and fisheries in Chapter IV. We added Appendix J, which displays wildlife species that are most likely to be affected by vegetation management activities. More material emphasizing the interactions of species and ecosystem stability is included in Chapters III and IV. Available literature concerning herbicide testing related to effects on wildlife and fish is highlighted. A more detailed discussion on "inerts" and their potential effects on wildlife is also included.

20. We added the Clean Water Act to the list of Major Legislation Relating to this EIS, Chapter I. Key legislation (for example, the Threatened and Endangered Species Act) were and are referenced in individual resource discussions in Chapters III and IV.

21. The beneficial uses of abundant water of good quality are listed in the section on Water Quality in Chapter III.

22. We agree. Information is incomplete on soil organism reaction to mitigation measures. We responded to these concerns in an expanded discussion in the Cumulative and Synergistic Effects section in Chapter IV.

23. Appendix J lists herbicide formulations with "inerts of concern" shown to display potential effects to wildlife and fish. Cumulative and synergistic effects are now discussed in Chapter IV, Wildlife, Riparian and Fisheries, Water, and Soils sections.

24. This concern is covered by the site-specific project monitoring requirements.

25. The term "Municipal Watershed" is not well defined.

25. Municipal watershed is now defined in the Glossary.

Issue: Interagency Coordination

Comments

1. Mitigation--Would Oregon Department of Transportation have to do an environmental analysis and human health management plan for each project or will the Forest Service do it?
2. Deal with discussion of shoulder slope vegetation. Chapter IV-81 says it prevents drainage from under the pavement, but no studies have shown this.

Responses

1. This is an implementation issue. The wording has been revised to state the requirements of the Forest Service in conjunction with other agencies prior to using any method of vegetation management.
2. Although specific studies have not been done on this, years of maintaining roads have shown associations between vegetation growth and pavement drainage. In the Chapter IV rights-of-way section we illustrate the relative "positive" effects of vegetation growth.

Issue: Other General Comments

Comments

1. All the errors of the Forest Plans have been imbedded in the VMT analysis. The Forest Plan process is fatally flawed. Therefore, so is the VMT analysis. This complaint includes the MR (formerly MMR controversy) that resulted in the latest round of DEIS supplements.

2. No alternative is presented that exhibits the current RPA program (36 CFR 219.12(f)(6) or market opportunity emphasis.

3. Provide the information contained in Figure S-3 on a National Forest level so we can analyze its effects on our community.

4. References, rather than actual numbers, were used in the document.

5. Alternatives should be selected by sub-regional, rather than region as a whole.

Responses

1. We added a section in Chapter IV to explain our rationale and the method used to develop the VMT economic analysis, and have worked to improve projections and calculations in the EIS. It is important to remember, however, that this EIS will not set the allowable sale quantity (ASQ) harvest level for any Forest, or for the Region as a whole. It is designed to portray for the decisionmaker the estimated level of effects so that the Alternatives can be compared.

2. Allocating resources to meet RPA goals is outside the scope of this EIS. This EIS covers processes and methods to implement a vegetation management program. Vegetation management serves as one of the tools in resources management as prescribed by the land management planning process.

3. We were not able to analyze all effects on a Forest-by-Forest basis, but we have now included timber yield and economic effects for each Forest. See Chapter IV.

4. The reference alternative is now displayed with actual numbers. Numbers could not be published earlier due to the then-unpublished state of most draft forest plans.

5. The scope of the EIS is regional. The EIS establishes a process to use region-wide in selecting actual treatments. The process is flexible, and treatments vary according to circumstances and site characteristics.

6. It is important that the purpose and theme of Alternative D include a clear statement of the components of an IPM program, which are: A determination of the economic or aesthetic injury level--that size of the pest population correlated with an injury sufficient to warrant control actions.

7. If a "last option" concept is used, a strict definition is imperative. Any definition of this concept should use cost-effectiveness as a strong determining factor for using herbicides since budgets ultimately control how much vegetation management work is accomplished.

8. Use of the proposed Forest Plans' preferred alternatives as a reference is illegal. Include a correct, no-action alternative...to compare the existing forest plans and management with the proposed management.

9. We think appropriate vegetation management action/nonaction should be based on on-site considerations.

10. Prevention, ecosystems approach, IPM concepts, etc., of Alt D should be part of all alternatives.

11. The DEIS is difficult to read and follow.

6. Using the five-step project planning process described in Chapter II, the basic concepts of integrated pest management (IPM) will be part of our implementation regardless of the alternative selected. Acceptable "injury levels" will be determined by site-specific analysis rather than by setting unilateral levels for the entire region.

7. We wanted public response to help us develop a definition for "herbicides as a last option". We now define it such that all other methods would be preferred to using herbicides, and would be used unless they were impractical or would generate unreasonable costs. We've added this definition to the text describing Alternative D.

8. We now compare the alternatives (Chapter II) with actual data estimates of effects, as well as how they differ from Alternative B. We still believe Alternative C is the truest "no action" scenerio. In our current situation, we do manage competing and unwanted vegetation, except that herbicides are not being used.

9. This EIS establishes regional policy and a process in selecting treatments. Flexibility remains at the Forest level in selecting appropriate treatments for specific sites.

10. These approaches are included in Alternative H, the preferred alternative, as well as in Alternative D. Not including them in other alternatives provides a broader range from which to choose.

11. The EIS certainly is large, technical and complicated. In developing the final, we have tried hard to make it more readable and easier for the average person to understand.

12. The projections of any effect to the Region, as a whole, masks possibly very significant subregional impacts.

13. The alternatives may unnecessarily limit local discretion and flexibility in the use of the various vegetation management methods.

14. The term "unwanted" is unwanted. The title of the document is biased and incorrect; not all vegetation is competing and unwanted.

15. Management of vegetation, fire-dependent plants, etc., in wildernesses is not addressed.

16. How will the Forests get specific direction?

17. Possible benefits of logging residues are not considered.

18. What about IPM? How does it fit in the selected alternative?

12. We broke allowable sale quantity down Forest-by-Forest. However, further breakdown of analyses did not appear to be warranted. It would have made very little difference in the decision, hence was not worth the extra expense.

13. NEPA directs protection of environment, health and safety, and social and economic base. Site specific analysis supports the 5-step process.

14. We kept the term "unwanted vegetation". Unwanted vegetation is that which competes with or poses a hazard to resources for which we are primarily managing on a particular site.

15. Wilderness management is not within the scope of this EIS. This is explained in Chapter I.

16. Forests will receive specific direction for monitoring in an implementation plan. The plan also will discuss growth loss, mitigation measures, and calculate risk and set threshold levels for vegetation management. Direction for cooperation with weed boards, as well as noxious weed management, prevention, damage and growth loss will also be contained in the plan.

17. The benefits of logging debris are discussed in Chapter IV under Subregional Timber Yield Effects.

18. Basic IPM (Integrated Pest Management) will be a part of implementing any alternative chosen (except the no-action, Alternative C).

19. We are concerned about maintaining all options and all tools.

19. Alternative H, the preferred alternative, does not prohibit the use of any particular tool. Specific herbicides are restricted. However, chemical methods are available when they are determined to be appropriate. See Chapter II, description of Alternative H.

Appendix I/A

**Public Participation
and Consultation**



Section 4

List of Respondents

A

Agricultural Resource Center
Aiken, St. Louis & Siljeg, P.S.
American Fisheries Soc/Oregon Chpt
Ashford Maintenance & Repair
Asotin County Sportmen's Assoc.
Asotin County Wheat Growers Board
Assoc Of Bainbridge Communities
Associated Oregon Loggers
Association Of O & C Cunties
Association Of Washington Business
Audubon Soc/Lower Columbia Basin
Audubon Society / Seattle Chapter
Audubon Society / Siskiyou Chapter
Audubon Society Of Central Oregon
Audubon Society Of Lane County
Audubon Society Of Portland
Audubon Society, Black Hills Chpt.
Audubon Society, Olympic Peninsula
Audubon Society, Pilchuck Chapter
Audubon Society/Tahoma Chapter
Avison Lumber Company

B

Bandon Rotary Club
Barbee Mill Company, Inc.
Bend, Oregon, Water Division
Bloedel Timberlands Development
Blue Mountain Resource Council
Boise Cascade/Timber & Wood Prod.
Breitenbush Retreat Center
Buse Timber & Sales, Inc.

C

C & H Reforesters
C. C. A. P.
Calif.Reg. Water Quality Ctrl. Bd.
Cascade Head Ranch Dist.Improv.Co.

Cathedral Forest Action Group
Cavenham Forest Industries, Inc.
Chamber Of Commerce Roseburg Area
Chamber Of Commerce/Wallowa County
Chamber Of Medford/Jackson County
Champion International Corp.
Ciba-Geigy Corporation
Citizens For Perpetual Resources
Citizens Task Force-T.S. Review
Coalition For Wise Forest Use
Columbia County Farm Bureau, Inc.
Columbia Plywood Corporation
Concerned Citizens Western Lane Co
Coos Head Timber Company
Cottage Grove, City Of
County Cattlemen's Assoc/Asotin Co
County Commission/Benton County
County Commission/Clallam County
County Commission/Curry County
County Commission/Douglas County
County Commission/Josephine County
County Commission/Kittitas County
County Commission/Klamath County
County Commission/Klickitat County
County Commission/Lake County
County Commission/Lincoln County
County Commission/Marion County
County Commission/Wallowa County
County Commissioners/Polk County
County Conservation Dist/Asotin Co
County Court/Grant County
County Court/Union County
County Crop Imprv./Garfield/Asotin
County Extension Agent/Douglas Co.
County Extension Agent/Union Co.
County Extension/Columbia County
County Extension/Wasco County
County Noxious Weeds/Garfield Co.
County P.U.D. No. 1/Clallam County
County Soil&Water Consv./Grant Co.
County Soil&Water Consv/Umatilla
County Weed Control/Asotin County
County Weed Control/Ferry County
County Weed Control/Klickitat Co.

County Weed Control/Lake County
 County Weed Control/Morrow County
 County Weed Control/Okanogan Co.
 County Weed Control/Tillamook Cty
 County Weed Control/Umatilla Co.
 County Week Control/Baker County

D

D R Johnson Lumber Company
 Deerlodge Forest Defense Fund
 Defenders Of Wildlife/Pnw Office
 Del Norte Taxpayers' League
 Don Whitaker Logging, Inc.
 Douglas County Lumber Company
 Douglas Timber Operators, Inc.
 Dow Chemical Company
 Du Pont

E

Earth First!
 Earth First! Seattle Chapter
 Earth First! Siskiyou
 Eugene Sand And Gravel, Inc.
 Evergreen Helicopters, Inc.

F

Farm & Home Center
 Flightcraft Inc.
 Flying Scotsman Inc.
 Fred S. James & Company Of Oregon
 Friends Cathedral Forest/Jennifer
 Friends Of The Earth
 Friends Of The Greensprings
 Frosty Hollow Nursery
 Fullerton Beverage, Pepsi-Cola Co.

G

Georgia-Pacific Corporation

Glide Lumber Products Company
 Golden Eagle Crop Dusting, Inc.
 Grande Ronde Resource Council

H

Habitat Creations
 Headwaters
 Heliotrope Natural Foods, Inc.
 High Fidelity Trees
 Hood River County Forest Manager
 Hood River County Forestry Dept.
 Hull-Oakes Lumber Company

I

Industry Task Force On 2,4-D Data
 Inland Empire Paper Company

J

John Kopplin Estate
 Johnson & Johnston Construction Co
 Johnson, D.R. Lumber Company

K

Klamath County Chamber Of Commerce

L

Lakeview Water Users, Inc.
 Lexington Grange #726
 Lilly Research Laboratories
 Linnton Plywood Association
 Lummi Indian Business Council

M

Mazamas
 Methow Valley Foods

Ministry Of Forests And Lands
Monsanto Agricultural Company
More Logs Inc.
Morgan & Engle Co./ Boss Logging
Mother Pearl Mushrooms
Mt. Baker Plywood
Mt. Rainier National Park Assoc.

N

Nat.Coal.Against Misuse Pesticides
National Assoc Conservation Dists
National Forest Products Assoc.
National Wildlife Federation
Native Plant Soc/Oregon/Emerald Ch
Native Plant Society Of Washington
Native Plant/Oregon/Siskiyou Chpt.
Native Plant/Wash./Vancouver Chpt.
Nature Conservancy
NCAP
New Bridge Grange #789
Nordic Plywood, Inc.
North West Timber Association
Northwest Chemical Corporation
Northwest Forest Resource Council
Northwest Forestry Association
Northwest Integrated Pest Mgmt.

O

Ochoco Lumber Company
Okanagan County Farm Bureau
Okanagan County Cattlemen's Assoc.
Okanagan County Farm Bureau
Okanagan Valley Chapter Bch
Olympic National Bank
Olympic Penninsula Citizens A.T.S.
Oregon Cattlemen's Association
Oregon Department Of Agriculture
Oregon Environmental Council
Oregon Farm Bureau
Oregon Forest Industries Council

Oregon Hunter's Assoc/Baker County
Oregon Parks & Recreation, Reg. 2
Oregon Parks&Recreation, Region 2
Oregon Salmon Commission
Oregon Society American Foresters
Oregon State Highway Division
Oregon Wheat Growers League
Oregon Women For Timber
Oregon, State Of
Oregonians For Food And Shelter
Osu Extension Service
Outdoors Unlimited, Inc.

P

Pacific Lumber And Shipping Co.
People Urging New Choices
Philomath Area Chamber Of Commerce
Plum Creek Timber Co, Inc.
Point No Point Treaty Council
Port Blakely Tree Farms
Portland Water Bureau
Puget Sound Plywood, Inc.

R

R.O.A.D.S.
Reservation Ranch
Resources Agency Of California
Ridden Farms
Rosboro Lumber Company
Roseburg Paving Company
Roseburg Resources Company

S

Salem, Or Water/Wastewater Dept
Salmon River Concerned Citizens
Save Our Ecosystems, Inc.
Scotsman Timber Co.
Seattle City Light

Sequoia Forest Industries, Inc.
 Sierra Club/Cascade Chapter
 Sierra Club/Oregon Chapter
 Sierra Club/Public Lands Committee
 Sierra Club/Puget Sound Group
 Sierra Club/Rogue Group
 Simpson Timber Company
 Simpson Timber Company
 Siskiyou National Forest
 Skyline Logging Company
 Small Woodland Services
 Snow Mountain Pine Company
 Solar Oregon
 Soncap
 Southern OR Citizens Against Toxic Sprays
 Southern Oregon Resources Alliance
 Southern Oregon Timber Ind. Assoc.
 Southern Pacific Transportation Co
 Southern Pacific Transportation Co
 Springfield Area Chamber Commerce
 Starker Forests Inc
 Sun Plywood, Inc.
 Superior Lumber Company
 Survival Center Association
 Swanson Bros. Lumber Co.
 Swanson-Superior Forest Products



Tacoma Public Utilities
 Tilth Producers' Co-Op
 Trout Unlimited Of Oregon
 Twin Rivers Logging Co



U.S. Environmental Protection Agcy
 USDA Forest Service/Applegate Rd
 USDA Forest Service/Umatilla Nf
 USDA Forest Service/Washington Off
 USDA Soil Conservation Service
 USDA Soil Conservation Service

USDA Soil Conservation Service
 USDD/Army Corps Of Engineers
 USDI/Bureau Of Indian Affairs
 USDI/Bureau Of Indian Affairs
 USDI/Environmental Project Review
 Vaagen Bros. Lumber, Inc.
 Wallowa Alliance
 Warm Springs Confederated Tribe
 Wasco County Livestock Association
 Wash. Friends Of Farms And Forests
 Washington Farm Forestry Assoc.



Washington Forest Protection Assoc



Washington Native Plant Society
 Washington Noxious Weed Control Bd
 Washington Soc. American Foresters
 Washington State Farm Bureau
 Washington Wilderness Coalition
 Washington, State Of
 Western Coating, Inc
 Western Farm Service, Inc.
 Western Forest Industries Assoc.
 Western Timber Company
 Western Wash. Toxics Coalition
 Western Wood Products Association
 Weyerhaeuser
 Weyerhaeuser Western For. Research
 Wilderness Society (The)
 Wildlife Society/Oregon Chapter
 Willamette Industries, Inc.
 Women Involved In Farm Economics



Gary Aaser
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Frank Abderhalden
Tom Able
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Peggy Abodeely
Erwin Abramson
Vic Abston
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Williams
Lupe Acosta
Daryl Z Adams
Gordon Adams
John C Adams
Phil L Adams
Robert E. Adams
Robert E. Adams
Tom Adams
Richard O Adamson
Ron Adkins
Milo Adkinson
Michael Ager
Lee Agidivis
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Thomas W Aichele
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Evangeline Andreason
Mark Andreason
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Cathy Austin
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Kennith Aynes
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E.R. Barton
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Gale Barty
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Melbern/ Annabel Bates
Opal Bates
Roberta Bates
Walter Bates
Irene Batey
Charles S. Battin
Ann S Bauer
M G Bauer
Alan Bauman
James Baumann

Teresa L. Baumann
Chris Baune
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William G Bayliss
Richard D Bays
Susan Baysinger
William C Beachman
Doug Beal
Jerry Beal
Lincoln G. Beal
M.F. Beal
Jack A. Bean
Raymond Bean
Burle Beard
Dan And Claudia
Beausoleil
Mr.& Mrs. Rolin Beaver
Phillip R. Bebout
Jennie Becerra
Beverly&Donald Beck
Peter S Beck
Ralph M Beck
Jim And Dotti Becker
Mark Becker
Vicki M. Becker
Jerry/Sharyn Becker/Boyd
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Warren Beeson
John E Beheytt
Fred Behm
Florence K Behrman
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Wayne Bell
Mickey Bellman
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Tom Bender

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Gerald Bendix
Gerhart Bendix
Gary Benedict
Robert E. Benjamin
Bettiana Benneth
John Benneth
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Clara Bennett
Emory C. Bennett
Richard S Bennett
Teresa G Bennett
Frank L. Bensel
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Stanley Benson
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Tom Bentley
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W W Benton
Bernard P. Bentz
Robert D. Berends
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Susan D. Berg
David Berger
Lyla Bergstrom
Roland E Bergstrom
Gene Bernards
Lee/Lucy Bernheisel/Reid
Jane Bernstein
Donna Berry
William C Berry
Joe L Berryman
Richard K. Best
Charles R Betts
Ronn Bevan
Bruce Beyer
Roy M. Beyer
John Bianco
Bruce Bice
Jim Bichell
Gerald Bichler
Gary Bickett
Carol E. Bickford

Robert E Bielenberg
Charlie L. Bierly
Kenneth&Evelyn Bierly
Myrtle Bigelow
Joanne Bigman
Joanne Bigman
John S Billings
Russell Bingaman
James E Bird
Richard Birkett
John A. Birnel
Lance Bisaccia
C M Bishop
Dan H Bishop
Howard A Bishop
James Bishop
John J Bishop
John M Bishop
Elf Bissinger
William M. Bitsas
Jerry Bitterman
Jerry Black
John W. Black
Bill Blackaby
Eric Blackledge
Sondra Blackwell
James R Blair
Frank & Helen Blake
Rosa M Blake
Gary W. Blanchard
Kenny Blankenburg
Dale Blankenship
James Blankenship
James A Blankenship
Talmadge Blankenship
Dave Blatchford
Keith, Et Al Blaylock
Larry Blem
James W. Blilie
Richard Bliss
Philip G Blohm
Alice P. Blossom
George/Nancy Blount

Mark L Blowers
Don Bockelman
Robert C Bockoven
Reinhold T Boeckel
Clifton H Boehmer
Shirley S Boehmer
Fernidand Boesch
Don B. Bohnert
Otto Bohnert
R W Bolander
Joseph E Boling
Jon Bolliger
Brian Dean Bollman
Jean Bolton
Tama Bolton
Scott Bonci
Ross Bondurant
Ferd Bondy
Wallace H Bonesteele
M E Bonime
Paul Bonneau
Geneva Book
Kathy Book
Gerald L Boom
John A Boone
Robert P. Booth
Stanley O Booth
Roger Boothe
Bill Boothman
Albert R Borges
Thomas L Bork
Jean Borland
John Borland
Tom Borland
Robert E. Borlen
Michael Bormuth
Thomas Boschma
Mark A Bosetti
Arlin L Bostian
Ben Boswell
Catherine Boucher
Dan Bourbonais
Lonnie A Bourgo

Gayle Bourns
David L. Bowden
Dorothy A. Bowen
Larry D Bowen
Cindy Bower
Julia Bower
Susan W Bowers
Rohmana E. Boyce
Charles A Boyd
Stephen Boyd
John Boyer
John M. Boyer
William T Boyer
Bryan M Boyl
Sharon Boysen
Don E Brace
Theresa A Brace
Joe Bradley
Lawrence M Bradley
Lincoln M Bradley
Doreen Bradshaw
Warren W Braley
Delbert Jeffery Branch
Al C Brand
Anne Brand
Michael E Brand
Vincent J Brand
John S Brandis
Brice L Brandt
Vernon Brandt
Gilbert Branstetter
Dave Brastow
Richard L Brathovde
Christopher Bratt
J J Bratt
Kenneth J Braun
T. A. Brauner
Bill Braunworth
Tatiana Bredikin
John F Breeden
Joann Breedlove
Ron Breedlove
Ellen Breiter

K. Breiter
Feena Breuer
Robert Brewer
Wayne L. Brewer
L G Brian
Stanley R Bridges
Don L. Briggs
Gary N Briggs
Sandra J Briggs
Dennis Bright
Ed Bring
Keith A Bristlin
Nelda Bristlin
Jo Broadwell
Thomas M Brobst
Richard Brock
Ivan & Alberta Brockamp
Frank Brohee
H. J. Bromley
Vivian Bronec
David Brooks
Dennis Brooks
Duane Brooks
Liz Brooks
Marilyn Brooks
Kristine R. Brotherton
Jim Brougher
Dave Browitt
Ann Chadbourne Brown
Bobb F. Brown
C L David Brown
D Brown
Dennis G Brown
Edward O Brown
Jack R Brown
James M Brown
James P Brown
Jim Brown
John A Brown
Leland H. Brown
Ray P Brown
Ray S Brown
Robert Brown

Rocky Brown
Rodney L. Brown
Sam E Brown
Thomas L Brown
Sally Browne
Toni Browning
William Brownsburger
Debbie & David Brozik
William Brubaker
Gary Bruce
Robert H Bruce
Calvin Brunabend
Bob Bruton
Jay L Bryant
Elizabeth J. Bryer
Dennis Bryson
Milissa Buchanan
Rohanna Buchanan
Richard Buck
Stuart Buck
B E Bucklin
Warren J Buckovic
Nancy A. Budge
Thomas W Budge
Robert G Buehler
J. F. Buehr
Delbert R Buell
Steven Buena
Arthur C Buether
William B Buffalo
Paul E. Buffam
Timothy A Buhler
Donald O Bullard
Charles W Buller
Mitchel A. Bulthuis
Michael J Bunch
James D. Bunker
Robert J Bunker
Thomas Burdge
Nancy Burford
Carolyn M Burgdorf
Kathryn Burger
Norman F Burgess

Don Burlingham
Scott Burlingham
Charles O Burnet
Cecil A Burnette
Mr&Mrs Fred Burnette
Budd G Burnie
Carol Burns
Alvin E Burr
Eric Burr
Marvin Burr
Shirley Burr
Michael D Burrell
Larry C Burrill
Michael E Burrill
F O Burrows
D Buse
Shannon Buse
Dwayne Bush
Thelma B Bushnell
Tim Butler
James Butsch
C. Bweled
Jeff Byrd



Joseph F Cacka
Leonard Owen Cade
Gary Caen
Grace D Caldwell
John C. Caldwell
Joseph E Caldwell
Muriel M. Caldwell
Steve Caldwell
Robert Calhoun
Christopher Calise
John M Callicrate
Augusta Callistini
Gary Calmettes
Charolette A Cameron
Christen A Cameron
Ken Cameron
Robert H. Cameron

Steven S. Cameron
Joli Campbel
Charlotte Campbell
David M. Campbell
E S Campbell
Harold A. Campbell
Karl & Patricia Campbell
Ronald S. Campbell
Ronald W. Campbell
Mel Canal
Keith A Canciti
Robert E. Cannaday
Thomas C. Cannon
Russell L. Canon
John D Cantlon
Richard C. Cantrell
Susan Cantrell
J.C. Capland
Miles J. Caples
Edgar L Capps
Gary Capps
Mary B. Capps
Joe Capshaw
Yvonne Cardemil
Ellen M Carey
Greg Carey
William Carey
Robert R. Carlon
Donald C. Carlson
Patricia A Carlson
R.L. Carlson
Steve L Carlson
Vance Carlson
Don And June Carlton
Mark Carmichael
The Carnahans
Erling E Carnes
Lawrence Carney
William A. Carney
Gary L Carr
Jay Carr
May Carrell
Vanelle Carrithers

Hortense Carrizales
E. Elizabeth J. Carroll
Mary Lou Carroll
Benjamin F. Carson
John Cartales
D.E. Carter
Greg Carter
Paul Carter
Steve Carter
Vernon D. Carter
Bruce Cartmel
Dale L Case
Harry E. Case
William M Case
Wren Case
Burl C. Cash
David B Casper
James H. Cassell
Tina Castanares
Anthony Castiglione
Robert N Catlett
Dale Catt
Richard Cauthorm
T R Cauthorn
Ann W. Cavanagh
Dalice Cdapshaw
Mayche B. Cech
Richard A. Cech
Doris Cellarius
Mildred E. Chadderton
Bill Chadsey
Virgil/Orella Chadwick
Amanda M Chagi
Charles M. Chambers
David H Chambers
A W Chamness
Charles Chandler
David M Chandler
Joe Chaney
Allen Chantry
Bruce R. Chapin
E. E. Chapman
William E Chapman

Floyd A Chartier
Charlie Chase
Rodney Chase
Glenn E. Chastain
Keith D. Cheney
Dale K. Cherney
Christine Cherrington
Jerome J. Chetock
Lawrence D Cheyne
Charles M Chinn
Don E Chitwood
Dan Christensen
David Christensen
Don Christensen
Stanley R Christensen
Vern Christensen
Closen F Christian
David A. Christian
Mollie Christian
Dale Christianer
Harold Christiansen
Harry Christiansen
Becky Christianson
James A. Christianson
John L Christie
Joseph Christie
Ronald E Christison
William J. Chronister
Joe J. Chuatal
Josephine E. Ciak
Calvin Bruce Clack
Don Claeyss
Paul Claeyssens
Darrell A. Clapp
Benjamin T. Clark
Betty Clark
David A. Clark
Hazel N. Clark
Kayla J Clark
Lindle D Clark
Marlin Clark
James M. Clarke
Jim Clarke

John Clausen
Sheri Clemen
Charles Clement
Tom Cleveland
Elmer L. Cline
Ralph N Clinton
Richard Cloepfil
Curtis R. Close
Melvin Cloud
Keith Cloudas
Don Clough
Paul Coates
V.E. Coates
Chester C. Coats
Donald C. Coats
Durwood Coats
Kenneth Cobleigh
Abner A. Coblentz
Dale Coblentz
Robert E Coburn
J. B. Cochran
George Cockrum
Hubert W. Cockrum
Kent Coe
Louis Coelho
Robert D. Coffelt
David Coffman
John Cogan
Judy Cohen
Tina Cohen
Nan/Richard Cohen/Gold
Richard T. Cohn
Tom Coiner
Phillip R. Coker
Steven B. Cole
Mary H. Colee
Brad Coleman
Chris Coleman
Joseph Coleman
Lee Coleman
Susan Coleman
Timothy Coleman
Bill J. Collar

Collard
James Earl Colley
Charles R. Collins
Marianne Collins
W E Collins
Stephanie Colony
Frank J Colton
Lily F Colton
Dick/Chris Colvard
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Keith Colvin
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Warren Van Mechelen
Fred Van Natta
Carl Van Orman
Gary R Van Orman
Al Van Proogen
Randal Van Prooyen
Ted Van Veen
Dave Vanasche
Herman Vandehey
Max Vannoy
Max W Vannoy
Julie Van'T Hul
John Vardanega
Ole E Vaslev
Lyle H Vassallo
Frank Vaughn
Charles Vaughn
Robert E Veatch
R A Bob Verburg
Barry S Verner
Kerment C Verner

Howard Vernon
Mike Verser
Joe Verstappen
Wali David Via
Mary & Joseph Victorine
David T Vincent
Wayne P Vincent
Ted Viramonte
Johan Visser
Judith Wright Visser
William Voelker
Gordon K Voget
Charles Volz
Leroy J. Volz
Alan William Von Borstel
Donald Von Borstel
Fred Von Borstel
George Von Borstel
Ronald Von Der Hellen
L E Vonderahe
Robert W Vorce
Robert W Vorce
Susan Vovon
Jim Voyles
Ralph A Vranizan
L. F. Vredenburg

W

Leslie A. Waber
Debbie R. Waddell
Joe Waddell
Robert W. Waddell
Bernice E Waddle
Cliff Waetjen
Donald R Wages
Dieter Wagner
E A Wagner
Gale Wagner
Jeff N Wagner
Patricia W Wagner
Verneda L. Wagner
Connie Wahl

James Wahlstrom
William W Wahrmund
Joey Waite
J L Wakefield
James L Walden
J. R. Walders
Ilene Waldorf
S T Waldrip
Betty Walker
Brent C Walker
Brent C Walker
Charles V Walker
Dale Walker
Donald E Walker
Harmon Walker
Jack Walker
Joseph Walker
Richard A Walker
Ron Walker
Sheila Walker
Warren H Walker
Michael J Wall
Sammuel E Wall
James M Wallace
Fred Wallender
R. T. Walls
Gary Wallstrom
Don Coin Walrod
Jim Walsh
Russell J Walsh
Edward L Walter
C.B. Walters
Lane Walters
Richard Walters
David Walton
Delton Walton
Jack Wanek
Tom Wanner
Alvin Ward
Charlene W. Ward
Craig Ward
James A Ward
James D. Ward

W M Ward
W. E. Ward
Wayne Warner
Wayne & Sandra Warner
Lynn E Warren
Robert E Warren
Ken L Washington
Stan Washington
Jack Waters
Jimmy L Watkins
W Grant Watkinson
R G Watson
R P Watson
Charles D Wavra
Kenneth L Waymire
Marion T. Weatherford
D Carey Weatherly
W. H. Weatherly
Ric Weatherman
Carl Weathers
Douglas Weathers
James V Weathers
Brad Weaver
Freda P. Weaver
Gay L Weaver
Joyce I Weaver
D Webb
Darell E Webb
Steve Webb
Frank S Weber
Mary Jo Weber
Mickey Weber
Ginger Webster
Rebecca Weeks
John H. Wehrman
Bob Weigand
Jeffrey A. Weilsen
John A Weimar
Al Weiner
Gerald J Weis
Robert Weisenback
Steve Weishaar
Duane Weiss

Rick S. Weitman
Hale Lee Weitzman
James M Welden
James J. Weldon
Patricia M Welk
Glenn Welliver
Allen Wells
Elizabeth L Wells
Gowlan Wells
Grover P Wells
Norman E Wells
James/Joseph Welsh/De
Franco
Bruce H Wendland
D E Wendland
Glenn Wendler
Karen Wennlund
John W Wensorski
Mark A Wentzel
Dean & Patricia Werth
Dennis R Werth
Elmer M. Werth
James L Werth
Betty L Wesley
Nelson Wesley
Don Wessel
Niels West
Tom And Tonya West
Hank Westbrook
Gordon S. Westergard
Gary L. Westerlund
Robert H Westfall
Samuel Bob Westin
Duane Weston
Rawlin Westover
Ken Wetgen
Frank M. Wetherbee
Percy J Wetherbee
Lee K Wetzal
Tom Whalen
Stuart T. Wharton
Everett W. Whealdon
Jon Wheeler

Micheal D Wheelock
Michael S Whelan
Elbert J Whitaker
Alton W White
C. White
Ernest E. White
Jeff White
Johanna White
Ken White
Leslie A White
Michelle M. White
Richard M White
Will J White
Allen E Whitney
Calvin Whitney
Sam M Whitney
Walter L Whitson
W B Whittemore
Noyes Whitten
Dale L. Wick
Marjorie Wick
Larry A Wicker
Stephan H Wickham
Bob Wienert
William T Wierman
Tracey/Bruce Wiese/
Carter
Beth Wieting
Karen D. Wilczek
W O Wildman
Carl R. Wiley
Robert/Patricia Wiley
Denny Wilfong
Chuck Wilkerson
G. Wilkins
Arlie A Wilkinson
Don K Wilkinson
Ruth Willard
Faith M. Willcox
R D Willhite
Arlene B Williams
Bud Williams
Daniel Williams

E. D. Williams
Fred S Williams
Geneva M. Williams
George Williams
John O Williams
John W Williams
Larry E. Williams
M L Williams
Richard W. Williams
Robert L Williams
Maurice Williamson
William D. Williamson
C H Willison
Alan L Wilson
Art Wilson
Bob Wilson
D H Wilson
Donald Wilson
Doris V. Wilson
Freddie D Wilson
Glenn A Wilson
Henry A. Wilson
James D Wilson
Jerry L Wilson
John Wilson
Marion Wilson
Nelson E Wilson
Patricia Wilson
Roberta Wilson
William A. Wilson
William K Wilson
Neal Wilt
Leland K Wimberly
Tom Wimberly
Gordon B Winchell
William E Windes
Preston Winn
Jason Winnett
William E Winsted
Doyle R. Winston
Steven A Winston
Arthur C Winters
Jack Winthers

Glory N Wipfli
Walter Wipfli
Al Wirth
Donald R Wirth
William V Wise
Susan L Wiseman
Patricia J Witt
Lowell Gene Wittlee
Charles A Wittnam
Carol M Wix
J Robert Wix
Philip E Wolf
Wayne S Wolfe
Oliver G Wolff
Lewis M. Wolk
Brian Wong
Deanna M Wong
Constance G Wood
Dale A Wood
Dale R Wood
Don Wood
Gordon T Wood
Karen V Wood
Kelsey Wood
Lewis O Wood
Louis A Wood
Louis A. Wood
Ruth J. Wood
Thurman C Wood
Lisa Woodard
Steve Woodard
Wade Woodard
Lauren T Wooddy
G. R. (Marie) Woods
Michael Woods
William C. Woods
John W Woolcott
Isabelle S Woolcott
Robert G Woolcott
Thomas R. Woolcott
Neil & Milani Wooldridge
Blanche V Woolfolk
Edgar W Woolfolk

George Wooten
Cliff Wordal
Anne M. Work
Sharon L Wornick
Willie J Wornstaff
C. Ellis Worthylake
Albert Wright
Donald P Wright
Gayland Wright
Keith Wright
Mildred E Wright
Norma Jean Wright
Pamela Wright
Richard Glenn Wright
Richard M. Wright
Terry Wright
Paul R Wulf
Jacqueline J Wulk
Willfred Wulk
Don Wyant
Jill Wyatt
Clinton W. Wynn
John R Wyse
George W Wyss
Maryann L Wyss
Raymond C Wyss



Dale D. Yager
Mary Ann Yakabi
George Yates
Steve Yates
Gerald Yawn
Gene Ychastain
Mae Yih
O. C. Yocom
Cate Yocum
Carole A. York
Victor J. York
Brigg Young
Dan W Young
Esther Young

Harvey L. Young
Patricia D Young
Ronald L. Young
William R Young
Grant R Youngberg
Howard Youngberg
Milo R Yount
Lionel Youst
Juan Yraguen
Nicolas B Yugyen
Karen Yule
John A Yungen
Sol Yutzy



Wilbert Zabel
Cherryl W Zacharias
Robert N Zacharias
Eleanor Zahn
John Zalman
V L Zander
Edward Zarosinski
Karl M Zearfoss
M.A. Zeilinski
Frank Zern
Steven Ziegler
Alvin Zielesch
Gail Zielesch
Nancy Zierenberg
Lee Zigler
Stephan Ziglinshi
Fred Zimmerman
Harold Zimmerman
Kenneth D Zimmerman
R Dale Zimmerman
Scott Zink
Voe Zorn
Bruce Zuber
Jean L. Zuber
Marvin Zuber
Sharon Zuber
Michael J Zubriski

Allen S Zulauf
Darrel Zuleger
Rena Zurbrick
Ralph Zusman
L. P. Zuvela
Jeffrey W. Zwar
Ronald Zwart

44 Anonymous Responses

Appendix I/A

**Public Participation
and Consultation**



Section 5

Technical Reviews
General Human Health
Growth and Yield

Introduction

This section contains reproduced copies of the technical reviews of the general and human health material in the Draft EIS. Eleven eminent scientists evaluated all the material in the human health sections and in the appendices. These reviews were arranged through the University of Washington and Oregon State University.

Also included is a review of Appendix A, Growth and Yield, written by Oregon State reviewers.

Human Health Technical Reviews

Richard J. Bull	Associate Professor of Pharmacology and Toxicology, Washington State University, Pullman, Washington.
Richard A. Carchman	Professor, Department of Pharmacology and Toxicology, Medical College of Virginia, Virginia Commonwealth University; Associate Scientific Director, Massey Cancer Center; Richmond, Virginia.
Frank N. Dost	Doctor of Veterinary Medicine; Extension Toxicologist and Professor of Agricultural Chemistry, Oregon State University, Corvallis, Oregon.
Labat-Anderson, Inc.	Dr. David Brusick, Dr. Edward Calabrese, Dr. Richard Thomas, toxicologists, Arlington, Virginia.
T. Sterling, A. Arundel	Faculty of Applied Sciences, School of Computing Science, Simon Fraser University, Burnaby, British Columbia, Canada.
Daniel Wartenberg	Assistant Professor, Department of Environmental and Community Medicine, Robert Wood Johnson Medical School, Piscataway, New Jersey.
Francis W. Weir	Ph.D, Diplomat of the American Board of Toxicology, Certified Industrial Hygienist; Health and Sciences Center, University of Texas, Houston, Texas.

Richard Wilson	Energy and Environmental Policy Center, Harvard University, Cambridge, Massachusetts.
Wallace D. Winters	Professor, Pharmacology and Anesthesiology, School of Medicine, University of California, Davis, California.
Shelia Hoar Zahm	Doctor of Science; Epidemiology and Biostatistics Program, National Center Institute, Bethesda, Maryland.

Growth and Yield Analyses

George W. Brown	Associate Dean for Research, College of Forestry, Oregon State University, Corvallis, Oregon.
Oregon State University	Research Assistants Daniel Opalach and Robert Gordon Wagner, and Senior Research Assistant Steven R. Radosevich, Forest Science Department, College of Forestry, Oregon State University, Corvallis, Oregon.

Washington
State University

Pharmacology/Toxicology Graduate Program, Pullman, Washington 99164-6510 / 509-335-8664

1/19/88

Mr. Gary L. Larsen, Group Leader
Vegetation Management
Forest Service, Pacific Northwest Region
United States Department of Agriculture
319 S. W. Pine
P. O. Box 3623
Portland, OR 97208

Dear Mr. Larsen,

As requested in your letter of Jan. 6, 1988, I have reviewed the draft environmental impact statement "Managing Competing and Unwanted Vegetation". The authors of this document should be complimented for a completing a truly encompassing document. While I will point out areas that I have some disagreement with below, I believe the overall approach attempts to be even-handed in its treatment of the alternative approaches.

My understanding of the intent of the document is as follows: It describes and evaluates alternative approaches to a program of vegetation management in 19 National Forests in the Pacific Northwest Region. The potential impacts of these approaches is evaluated in terms of environmental quality, social perceptions and potential health effects and in view of the benefits that result depending upon the relative effectiveness of these alternatives in controlling competing and unwanted vegetation. The actual alternative to be employed at a given site is to depend upon an analysis of the benefits and impacts at that site.

Seven alternatives are described. The present review only deals with the approach that is described for evaluating the human health effects that might be associated with each alternative. The risks most directly addressed are those that involve the use of herbicides, but do address problems associated with burning as a control method as well.

As a general comment, I have not attempted to correct typographical and/or grammatical errors in the document unless they lead to a substantial confusion in interpretation. Such errors do exist and a technical editor should review the entire document.

Comments specific to sections of the document:

p. II-26, Table II-3

The units used in the risk indices for herbicides and smoke are not clearly defined in this table or the associated text. The numbers apparently relate to the number of acres that are treated, which I find to be generally appropriate. There are corrections made to the figures and a differential developed for

public and worker hazards. However, it is not clear how this translates into actual human risk, nor how these hazards relate in a quantitative way to other estimates of hazard (e.g. accident rates per 1000 acres that are manually treated). As long as the comparison only considers the relative hazards associated with smoke and herbicide applications between the options, there is no difficulty. However, as these estimates are used to estimate overall hazards associated with a given alternative management scheme in section IV, it is difficult to determine how to weight hazards from smoke and herbicide with other types of risks which are encountered.

p. iv-9 and 10.

The notion that "costs of advancing the state of the art sufficiently to allow characterization of the human health risk due to smoke are inestimably large, and therefore exorbitant" is a smokescreen itself. That statement can as easily be made about almost all of the risk estimates made in this document in the sense that more data will always be useful in assessing risks. Certainly, better bounds on the risks due to fire smoke could be obtained at relatively small incremental costs. For example, an animal study that establishes the relative hazard of inhaled smoke from material burned in these situations compared to more thoroughly studied smokes would certainly improve the estimates of risk. Additional incremental improvements in the risk estimates could be added through study of the dispersal of key ingredients of such smoke under actual field conditions, etc.. I am very comfortable with the argument that there are direct costs of delaying action until more information is obtained. Too often, however, this is simply an excuse for never developing the appropriate information.

The number of acres treated by alternative methods is a first indication of the relative health hazard associated with application of herbicides or from smoke. However, I believe that the size of the population that would be affected would be a useful modification of the approach as it is applied to a particular site.

p. iv-87

Malignant tumors do not invade normal cells, they invade tissues.

p. iv-88

The statements made in the second paragraph from the bottom is a significant departure from methodology generally applied by other Federal agencies. Ordinarily NOEL and LOEL are terms used only for applying safety factors for non-carcinogenic health effects. The term LOEL is not ordinarily used in estimating carcinogenic risks. Part of the confusion seems to come from the use of Ames' HERP approach where he apparently uses an estimate of a dose that produces a 50% response rates in rodents. This is

a derived figure that differs significantly from a LOEL. I discuss the implications of this approach in greater detail below.

This section further states that "a one-hit regression model" and the upper 95% confidence interval for effect.... was used. I could not find any indication that this approach was actually used in the comparisons (i.e. the document seems to depend on a LOEL HERP) although there are tables in Appendix H which display these data. If this does impact the assessment in any real way, I believe it should be made more apparent. Secondly, I think the use of the one-hit model needs to be more specifically justified, since EPA uses a somewhat less conservative approach with the multistage model.

I do understand understand that some difficulties in using certain immunological and behavioral indices in risk assessment. I am somewhat puzzled by that immunotoxicological and neurotoxic effects are summarily dismissed here. Surely this does not mean that a chemical that produces a peripheral neuropathy and for which sufficient data exists to define a NOEL would be excluded as a significant hazard? This seems to be what this implies. I fail to see how this would differ from systemic toxicities to other organ systems.

p. iv-93 and 94

I particularly like the explanation of safety or uncertainty factors that is provided and the use of that method to characterize hazards under projected field situations.

p. iv-94

I am not convinced that the use of the HERP offers any substantial advantage over other methods of estimating carcinogenic risks. The major problem with the use of this approach is that it results in a dimensionless ratio that applies to a particular situation. It cannot be easily translated into different situations unless both of the data points from which the ratio is derived are known (i.e. the dose that gives 50% tumor incidence in rodents and the estimated human exposure). A measure of risk based on unit risk/per unit of dose is much easier to transfer to other situations and is less likely to be misinterpreted. I am also troubled by the fact that it makes minimal use of dose-response information that is available. This may be an academic point when most bioassays have used two doses and a control group. However, some circumstances exist where much better data does exist.

Table IV-27

I frankly don't like the use of the LOEL HERP. The Ames' HERP is based on an estimate of the 50% response rate and is much more defensible. It is a point that is defined through the use

of the entire dose-response curve. As a result it is a much more reproducible figure and is not so dependent on numbers of animals per group as is a LOEL. As a departure point for estimating relative hazard of a chemical carcinogen, the 50% response rate is obviously much superior.

A rather fundamental policy question should be raised here. Does the use of HERPs provide any substantial advantage over the extrapolation methods used by other Federal agencies. As I see it, the primary problem is that this departure from convention will confuse the public. Unless there is a substantive advantage for changing to HERPs that is not apparent to me, I would suggest remaining with the multistage model that is employed for quantifying carcinogenic potency by EPA. This method has the additional advantage of being more easily adapted to varying scenarios because it expresses hazard explicitly in terms of exposure. Cautions should be made that make it clear that the accuracy of such estimates leaves much to be desired and the figures should be viewed primarily as a means of comparing potencies of these chemicals as carcinogens.

p. iv.-122

A picky comment. BaP is not a particularly toxic PAH. It is, however, one of the more carcinogenic. These terms should not be confused in the document. Please note that the letters have been reversed to PaB in one instance. Finally, I think that the initial mention of BaP should explicitly identify this compound as benzo(a)pyrene.

p. iv-123.

In the face of inadequate data, the smoke from burning unwanted vegetation be assumed to carry the same chronic hazards that are associated with burning of fossil fuels at least for chronic health effects. Although the concentrations of individual constituents will vary, most of them (e.g. PAHs, aldehydes, oxides of nitrogen) will be represented in the smoke of burning vegetation. The chemicals identified as being present in negligible concentrations (e.g. sulfur oxides and sulfates) are primarily acute hazards. Although in the end, the analysis of risk from smoke essentially adopts the notion that these risks are equivalent (i.e. by saying that the effects are uncertain) it is not ever made clear that it is this comparison which makes it difficult to dismiss fire smoke as a chronic health hazard.

As an aside, this appears to be the area of greatest uncertainty in evaluating the health hazards with the different alternatives. I suggest that some toxicological and/or epidemiological evaluations of the long and short term effects of inhaling smoke from burning vegetation should be done simply for occupational safety reasons.

I would also like to provide the following responses to the general questions asked in the second page of your letter.

1. In general, the presentation of the health effects associated with the various options are well characterized. As directed, I have not gone beyond the actual document to determine if adverse health effects have been inadvertently omitted from the review.

I do have difficulties with the methodologies that were used for cancer risk assessments. There were at least two methods described, but the reasons for selecting the LOEL/HERP was not made clear. Given the choice of the HERP, the use of the LOELs for calculating HERPs is not defensible. Use of the TD₅₀ suggested by Ames would be preferable for the reasons discussed above. Personally, I would prefer that estimates of unit risk per unit dose be used rather than HERPs.

2. The statements that immunotoxicology and neurotoxicology endpoints cannot be used to estimate risks is of real concern to me. I do not know if such information would be critical for any of the compounds reviewed or not. However, frankly neurotoxic or immunotoxic (i.e. immunosuppression to the point that susceptibility to infection is demonstrably greater) can be handled like any other systemic toxicity.

A second area of concern was that the health hazards associated with smoke were removed from consideration because of lack of directly applicable data. The only risks from options involving burning seemed to be physical injury. While acknowledging the difficulties associated with the lack of data, I have a feeling that fire smoke may represent the largest uncharacterized hazard associated with any of the options in terms of chronic health effects.

3. The general approach of incorporating health risks into the context of other risks and benefits when several options are possible is an excellent idea. This type of comparison suffers, however, when potentially significant sources of health risk are obscured by lack of data (e.g. fire smoke).

4. Provision of formula for considering site-specific characteristics into an analysis of risks would seem to be an appropriate modification of this approach. From the health perspective, the proximity and size of human populations and/or the extent of recreational use during the period of treatment would provide for a better characterization of risks. If there is to be no human exposure, obviously options that minimize other adverse impacts on the environment should be chosen.

Despite indications that site-specific evaluations are to be done, the use of certain means of expressing risks does not

lend itself well to site specific evaluations (e.g. the use of HERPs rather than unit risk per unit dose).

I hope that you find these comments to be helpful. Please feel free to contact me if you have any questions.

Sincerely,

A handwritten signature in cursive script, appearing to read "Richard J. Bull". The signature is written in dark ink and is positioned above the typed name.

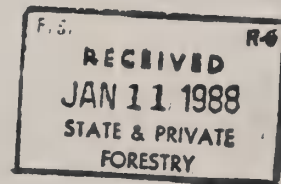
Richard J. Bull, Ph.D.
Assoc. Prof. Pharmacol./Toxicol.



Medical College of Virginia
Virginia Commonwealth University

January 4, 1988

Mr. Gary Larson
USDA Forest Service
Pacific NW Region
P. O. Box 3623
Portland, Oregon 97208



Dear Mr. Larson:

In accordance with the letter from Dr. Frank N. Dost (Nov. 8, 1987), I am providing a review and comments on the human health risk characterizations in Chapters 2 & 4 of the Draft EIS with respect to five major points:

1. quality of information available
2. methodology for characterizing risk
3. conclusions drawn
4. the use of the conclusions in planning
5. recommendations on risk management policies for workers and the public

In general, I feel that all five of the above points are developed in a scientifically defensible manner given our current state of knowledge. There are though a number of both broad and specific points presented in these documents which require further discussion.

Appendices: Comments: Appendix D: Human Health Risk
Assessment (Qualitative) - Section 1

In the overview section (p1, paragraph 2), I believe that a clear distinction should be made here between non-carcinogenic and carcinogenic risk. In addition, avoid the use of jargon wherever possible (e.g. lab p.1, paragraph 4 should read laboratory). Include in the uncertainty sections:

- a) rate of exposure,
- b) vehicle effects, and
- c) differences in xenobiotic metabolism and pharmacokinetics.

Indeed there are very many levels of uncertainty involved in extrapolating from animal studies to estimating risk to human health. In fact, there are so many assumptions that go into such deliberations there are scientifically credible individuals who would take umbrage with the use of quantitative for this or these approaches (estimate might be more acceptable). In those cases where risk assesment has been applied using human data (e.g. vinyl chloride), the actual cancer incidence from exposure to vinyl chloride is 10-100 times less than what the risk figures predict. How well these estimators work using animal data is even less certain. Recently, editorials and letters in Science (1987) have raised further questions concerning:

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January 4, 1988
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1. the use of animal studies to evaluate carcinogenic potential
2. that a greater area of risk outside the workplace may be with natural carcinogens - e.g. in food (see Ableson, R.H.: Ames, B.N.: Davis, D.L.:).

It might be useful to provide a more extensive discussion of risk per se such that a lay person might be able to place in better perspective what estimation of risk assessment means (e.g. compared to more generally understood risks) (e.g. Table 5-15 Driving a Car). In this year's Science, there was a series of articles that attempted just such a discussion. Dr. Nichols and colleagues at Harvard University have also published articles that relate to these issues. I realize the driving forces behind the use of these approaches, but stronger cautionary statements need to be included in this document.

On p.2, paragraph 4 - last sentence might be fleshed out some more [e.g. how much might these procedures exaggerate the risk (probably unknown)]? What is the effect(s) of exaggerated (unrealistic) risks - perceived/real - public costs etc. etc.?

Structure of the Risk Assessment (p3)

1. the herbicides under consideration are the commercial formulations not the pure material and as such it should be so stated. In the case of the animal studies every attempt should be made to emphasize, where known, whether the pure or commercial grade material was tested.
2. I suggest adding a sentence on pesticide (or more general) interactions in this section.
3. Tables and Figures of chemical structures - physical/chemical characteristics would be helpful for the 16 herbicides.

P4 - Hazard Analysis - Add both rates and levels of exposure, though people have been arguing (e.g. formaldehyde) that the level of xenobiotic at the target site should be what is used in the estimation of risk. Under chronic toxic effects, provide some reference or breakout of these. Under data gaps, refer reader to a section where these are identified in the EIS. Exposure analysis - change calculate to estimate doses. Risk analysis - not only should doses be compared but also the routes of exposure especially in the case of herbicides.

Table 1 (p6) should state that this data is based/year on p5, paragraph 2. It is not clear to me why treating 100,000 acres/year "makes the worst case assumption." If it's based on adding all the figures in Table 1-1 together, then this should be so stated. I think wherever possible the basis for these scenarios should be indicated.

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page 3

P8, paragraph - Why are synergistic effects of mixtures mentioned without some mention that this would represent a worst case scenario. In addition, other attempts to estimate these kinds of risks simply assume additive (i.e. non interactive) effects (USEPA).

The data gaps identified on p9 should be highlighted further. Item No. 6 (p10) is stated in such a way as to prejudice the reader. In addition, there are many different kinds of experimental designs which could be considered which might make these studies financially possible (e.g. central composite, Box-Benken etc.). The way this is written might suggest to a reader knowledgeable in this area that this statement reflects the thoughts of an uninformed writer.

Chronic feeding studies (p6, paragraph 2, section 3) - The use of the phrase feeding studies (i.e. Administration of compound as an admixture in the animals' food) should be changed to more clearly indicate what is going on. Paragraph 5 of this same p6 - The first sentence should more clearly state what the tests results are used for. You might also want to include somewhere in this document arguments relating to the relationships of these assays to carcinogenic endpoints (e.g. Rinkus & Legator, Cancer Research, 1979).

The use of mutagenicity and genotoxicity is almost interchangeable in this document. This is probably OK, but not all genotoxic endpoints are due to mutational events (e.g. recombination). Thereby, I would suggest some caution in using these terms interchangeably.

P16, section 3 Picloram - "EPA has determined that the positive study was insensitive..." Does this mean to suggest that picloram is even more mutagenic? I doubt this is what was intended, but please rephrase to more clearly reflect the problems with this assay which the USEPA apparently has rejected the data from.

P24, paragraph 1, section 3 - Was the least-squares linear regression procedure weighted or unweighted? If weighted, how so?

P24, paragraph 3 - Amitrole - Why is there an exception here based on the use of a threshold? This should be clarified further.

P25 Picloram - need to discuss and or reference the significance and controversy of benign vs. malignant tumors. This could be confusing with such an addition since the EIS states "picloram carcinogenicity..." You could argue another worst case scenario based on the assumption that these benign lesions could be premalignant, but without making such a point, this could be confusing.

A critical point is made and "buried" on p28, section 3, paragraph 4 -

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page 4

"The gap in the testing of the herbicides as formulated products, according to one view, gives rise to an inference that the environmental consequences, including hazards to human health, from using them are largely unknown." This point should have been included earlier under data gaps at least comparisons should be made between the pure and technical formulations in selected short term assays (e.g. Ames) to help address this key point. I agree with this document's competing viewpoint only in so far as no one has really done a meaningful comparison, but it still represents a gap and one that is testable.

P7, section 4, paragraph 1 - How was the residue on vegetables estimated and the amount of material ingested? I assume that washing of the vegetable and skin or peel removal as well as convoluted surface areas of vegetables was also "included" in this calculation. Whatever was done in this case should be available for criticizing. For this entire section, what is the probability of these events occurring?

P11, section 4 - I think that a very clear statement (including data) should be provided to allow for a better appreciation of the relationship between exposure/dose with different levels of worker protection. I indicate this in part because of the data in Table 4-4. I may have misunderstood the information included in Table 4-4. See Levy et al study wherein it appears that individuals with protective gear seemed to have higher average doses than less protected individuals. In addition, some statistical analysis of these data might be additionally revealing.

The use of the word "realistic" to describe a situation less extreme than a worst case (e.g. dose) could be misleading for those readers who do not delve into the definition (i.e. realistic). I bring this up for the following reasons:

1. someone might actually believe at face value that the word realistic as used is simply that.
2. the use of this word might then provide little room for adjusting exposure doses.

P21, section 4, paragraph 3 - Could you refer the reader to some reference as to the computer program? Are there competing programs that give different results? What other assumptions are made by this computer program?

P22, section 4, paragraph 1 and paragraph 2 - Is the consumption of 400gms of peas or berries and 1 liter of water on a daily basis? Why is 400gms have (0.9 lbs) and 1 liter has no equivalent? It's a minor point but should be consistent. In addition, the consumption of contaminated foods which will be consumed following cooking - does that also assume no loss of herbicide? This should be so specified. This document should also again indicate where the

Mr. Gary Larson
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assumption of daily fish consumption was obtained - same for other consummable items. Table 4-8 should indicate whether these are daily doses?

Section 5, p21, paragraph 3
What would the actual differences be between estimated cancer risks using the threshold and non-threshold models? Do you think that EPA's model use is really less appropriate? It might be instructive to allow the reviewer to compare the outcomes from using these models since there may appear to be some inconsistencies in so far as what procedures are used and then and what the outcomes would be using competing models with different assumptions. Especially confusing to me when I read paragraph 3, p22, section 5, practically speaking (item 4), which prediction models have been compared with actual observations of tumor incidence - e.g. vinyl chloride? My recollection is that observations of human tumor incidence does not agree very well with model predictions. Therefore, some scientists or policy/regulations might argue about the use of the word conservative to describe e.g. the one-hit model.

It appears from the way in which amitrole is discussed that (p25, section 5), it could (or should) be a good candidate for removal from use. How does a non "expert" in risk assessment use this information in "Cancer Risk to the Public" to evaluate the significance of this section? Table 5-15 does go a long way to dealing with this issue(s). Do you think it would be worthwhile to have the concepts which this Table conveys as part of an executive summary?

P30, Section 5 - Synergistic and Cumulative Effects

Why must synergistic effects result from simultaneous exposure? I would think that time should also be considered as a significant dependent variable. What do you mean by synergism? See Berenbaum (1981) for some significant thoughts and examples pertaining to this point. I believe the explanation provided is too simplistic. It also appears that from the statement that "synergistic" effects of herbicide combinations "cannot" be predicted based on the effects of the individual chemicals, "one should not be able to conclude that it is "unlikely that synergistic effects could occur..." I was unable to obtain a copy of the Kociba and Mullison (1985) article in time for my review so I have no way of evaluating their data presented in this section though the fact that 2-4D and picloram when tested at one dose (i.e. LC₅₀) of each compound suggest no synergism is only mildly comforting while the skin sensitization studies poses a situation that stretches the stated meaning of synergism in this document.

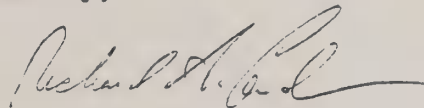
The above sections complete those specific points in the Appendices of the Draft EIS which were the basis for the human health risk characterization in Chapters 2 & 4 of the Draft EIS. Chapters 2 & 4 of the Draft EIS provided the reader with a very thorough, complete and rational approach to establishing a

Mr. Gary Larson
January 4, 1988
page 6

reasonable course of action. I think the approach taken here is excellent and should as stated in Dr. Dost's letter "become a model for such documents." These documents represent one of the best documents of its kind I have had an opportunity to read. My comments, criticisms and questions stated above in no way diminishes my enthusiasm for the work presented. I'm sure that many of my comments raise issues for which we currently do not have either the data for or an explanation of. In conclusion, this Draft EIS represents a responsible approach to an important area of concern and as such has my support.

I appreciate the opportunity of presenting some of my thoughts on these documents and look forward to seeing other views on these issues.

Sincerely,



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RAC:wls

SCIENTIFIC REVIEW OF HEALTH IMPACTS SECTIONS OF
DRAFT ENVIRONMENTAL IMPACT STATEMENT:
MANAGING COMPETING AND UNWANTED VEGETATION
U. S. FOREST SERVICE, REGION SIX, OCTOBER 1987

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January 15, 1988

Review of Health Impacts Sections of Draft Environmental Impact Statement

PART A: GENERAL COMMENTS ON THE DEIS

In this section are general comments that apply to the DEIS and its Appendices as a project, whereas Parts B and C refer to specific items in the DEIS and Appendices, respectively. Some of these concerns are matters of form and organization, others are substantive.

There is no question that this document has the potential to become a landmark in the long struggle to learn how to develop a proper environmental impact statement. That is not a facetious statement, because federal agencies have been trying to accommodate requirements of NEPA, the concerns of all of the interest groups in their constituencies, internal management needs and probably a host of other influences that all contain intrinsic sources of conflict.

It is evident that a massive amount of work has been invested in the project, and it deserves the best efforts of those who are examining it. I have tried to meet that obligation. It would be easy enough to point to passages that cause scientific annoyance and declare the document to be inadequate. Certainly there are passages that cause me great concern, either in their technical verity, or in their apparent expression of some pre-established end. Those occasions, singly or in sum, do not justify condemnation of a very productive outcome of the work done up to this point. I see my professional effort as an attempt to help make the document reach its intended goal.

The comments relative to the documents, overall:

1. It is very difficult to follow the necessary trail through information to the conclusions drawn by the document. Material is scattered, and it is not easy to backtrack at any given time to find the basis for end points.
2. It is possible that the problem of widely separated parts might be solved with a detailed index, which ought to be constructed regardless of any change in organization. The index should cover the entire document and appendices, and should appear in both volumes.
3. Related to the index, paging would be most manageable beginning and running all the way through, particularly in the main document. In the Appendices an alphanumeric system would probably work. As it is, it is easy to lose track of where we are in the books.
4. There are serious gaps in the glossary, several of which have been mentioned with regard to specific terms in the documents. I have not yet taken the time to examine the glossaries in detail; in fact, I am not sure I found them all. Aside from the instances where I found important terms used in the text to be missing, I examined page 10 of the list in Appendix D. Several entries are either inadequate or simply incorrect. A glossary is not an extract from a dictionary. It must be constructed as an adjunct to the context of the material it supports. The definitions of teratogenesis, thiourea, thyroid gland, toxicity, and volatility are either incomplete or miscast.

The entire document must be examined for terminology that should be included in the glossary, and a separate effort to assure proper definition in context is needed. I do not have the resource to do that as part of this review.

When this part of the document is reconstructed, all health impact related terms should be brought together and duplicated in both volumes.

5. The references are inserted section by section with no common style. The time compression toward the end of the preparation period is probably responsible in part. I found enough absences and errors to be quite concerned. I presume that a single standard format will be used. It seems preferable to assemble the entire reference list as a single section for the whole document, again placed in both volumes.
6. Roman and Arabic numbers are mixed at the same level of hierarchy throughout the work. It probably would be best to use Arabic, given the great number of sections.
7. These analyses are intended to incorporate all of the major decision factors into a matrix that leads to a rational and supportable decision. Even given the highly conservative assumptions used in estimating potential health effects, this has been done with most of the technical questions.

There are other influences on the decision making process that were acknowledged but not analysed, the most important of which is public perception. It is customary to assume that perception relates only to risk, but it is important in the application of all aspects of forest management and technology.

Coupled with absence of an analysis of the role of perception, there is no process identified for making decisions about vegetation management that are driven by perception or other nonquantifiable and nontechnical indices.

8. In the introduction to Chapter 4 of the DEIS, it is pointed out that it was necessary to rely on published reviews of studies in most cases. In Appendix H, on page 10, was the statement that "a comprehensive review of the open literature is beyond the scope of this project".

That position is unacceptable. A major university was contracted to assist with preparation of these documents. All of the skills and resources needed to bring the literature base up to the moment of closure, and integrate it into the DEIS were available. I have encountered several places where pertinent literature was not referenced or used, and I was not looking for such absences.

Reviews are useful, because they do integrate information, but they must not be used as authority for conclusions in original papers.

(See also comments related to Appendix H, page 10/2)

FD:mcc

Review of Health Impacts Sections of Draft Environmental Impact Statement

PART C: REVIEW OF APPENDICES D AND H

Subjects of these comments may be located within major headings by page/paragraph/line unless otherwise noted. A partial paragraph at the top of a page is paragraph one.

Comments of a technical nature are presented first and editorial matters are grouped at the end of this part of the review.

APPENDIX D

SECTION 1

1/3/5 Other forest workers than applicators may be subject to occupational exposure.

1/4/3 "Safe" levels are not established in a laboratory. The sentence beginning on line 2, " First----- effects," should be rewritten. In the following sentence, the word "additional" is used, and no prior safety factors have been discussed. NOELS are not safety factors.

2/1/4 I would suggest defining "safe" somewhere in this part of the document. I prefer to define safe, or safety, as the practical certainty that no harm will arise from a given use (or dose). There are other similar conventions.

2/2/6 Somewhere in the front in both volumes there should be a very bold statement of conventions, including the caveat expressed here, that the analyses in the document are based on estimated one time or few human doses compared with daily doses given animals over periods as long as a lifetime. These are well described in various places in the DEIS and should remain, but the condition should also be stated in a place of high visibility.

2/3 This paragraph is quite unclear. The first sentence should indicate that because it is not known that there is a threshold, and because it is biologically plausible that there may be no practical threshold, it must be assumed that there is none. Therefore, we must assume for purposes of regulation or risk assessment, that any small dose of a carcinogen has some probability of a carcinogenic effect. (See also Sect 3, 4/3/1)

The cancer potency can be defined a little better. It is a projected probability that cancer will be caused at a standard dose rate, usually one mg/kg/day over a lifetime. It is used by a linear comparison with measured or estimated doses in the field averaged over a working career or a life time. The "probability of developing tumors at increasing dose levels" is an expression of the dose-response curve, which is used all or in part to define potency.

3/2 Hazard, risk and exposure are not defined in the glossary. In some parts of the world hazard and risk mean just the opposite of the usage in this country, and there is often interchange of meaning here. They should be clearly defined at the point of first use, as well as in the glossary, because they are the most important conclusions of the entire exercise.

It seems to me that hazard analysis is a determination of the nature of the effects, which includes both the kinds of responses, and the dose response relationship. Why is chemical structure necessary to hazard analysis? Does it direct additional research, or is it used to estimate risk? I suspect that neither usage is really applicable in this particular context.

- 4/ fig.1. I must question whether the LD 50 has sufficient utility to list separately. It is subsumed under the first bullet, where it may be desirable to say, "including acute responses."
- 8/3/8 Estimated average daily exposure should be explained more completely. This statement leaves an impression that everyone is exposed continually, and doesn't make it clear that even a single dose would be averaged over a lifetime for the risk assessment.
- 8/4 There is no quantitative risk assessment for mutagens,. The convention used in this document, that cancer risk reflects mutagenic risk, is quite satisfactory lacking any better method.
- 8/5 The sentence on synergism is probably technically correct, but it tends to imply that there is actually a fair amount of useful direct data to appear later. In the rest of the document, the issue is discussed in the sketchiest of terms where it arises at all.
- 10/1 There should be some definition of just what the gaps really are and why they are important or unimportant to the DEIS. In the usage of this document, are they simply areas where EPA has required additional data, or are they areas in which there is a great deal of information already, that does not fit the pattern required by EPA, or is there just an absence of usable data? Using 2,4-D as an example, there is an immense array of work, some good, some not, but in sum it leads to a conclusion about mutagenicity that will probably stand no matter how much more work is done. EPA has requested more study, but the purpose is as much to bring the data base into conformity as it is to establish whether the chemical is in fact mutagenic. This is discussed on page 11, item 6, but perhaps it ought to be earlier.

SECTION 2

- 9/Item 1. The issue of zero wind should be addressed. No application near sensitive site should be made unless there is an active wind away from the site. There should be no aerial treatment near sensitive sites in dead air.

SECTION 3

- 2/4/2 The words, "metabolize" and "metabolism," should be in the glossary. Differentiate between metabolism of pesticides and metabolism of energy substrates. For this purpose it is not necessary to discuss metabolism of hormones, neurotransmitters and other endogenous non-energy producing substances, nor is metabolism in the sense of biosynthesis pertinent.

2/5, 3/1 These two paragraphs are not well separated and probably ought to be redrafted. They should include a clear discussion of dose-response relationships. In fact it would probably be best to include a separate paragraph on D/R. On page 3, the sentence beginning on line 4, para 1 should read, "Chemicals that cause cancer are generally assumed to possess no threshold, and even extremely small doses are assumed to have some finite probability of producing a carcinogenic response." The probability or risk, is related directly to the dose. (See Sec 1, 2/3)

3/3/10 Parenteral dosing does not bypass natural protective mechanisms. In fact the result may be to move some of the material to the liver through the portal circulation without the moderating effects of the gut content and dilution in gut volume. It is true that some portion of the dose will enter the venous drainage of the abdominal or thoracic wall or the subcutaneous circulation and will reach the systemic circulation before it is recirculated back to the liver or other sites of detoxication.

There are all kinds of possibilities associated with route of administration that may either increase or decrease response or have no effect, depending on the drug, the site, and the amount administered. The real reason that parenteral administration is not used is that it does not approximate field exposure, and it is impossible to perform properly over long periods with multitudes of animals

3/4/2 The weight of the animal alone will not give a conversion. A rough approximation can be based on standard consumption data for a given species and weight, or food consumption is monitored directly. The latter is usually done to account for unusual relations between weight gain and intake, and to provide accurate data on dosage per unit weight. The procedure for converting dietary concentration to dosage ought to be. Elsewhere in the document conversions are stated, with reference to the USDA 1984 background document, but with no mention of the method.

4/1 Acute toxicity studies are used for much more than determining LD50s. The LD50 isn't useful for much more than determining doses to be used for single dose studies, and as a first step in rangefinding for longer term work.

4/2 This is essentially a one sentence paragraph that is rather unclear. Among other things, no effect doses are developed for studies of all lengths.

5a Threshold is not quite correct in this usage. It is the point at which a response first appears on the dose response curve. As pointed out in Casarett and Doull (3rd ed, p19-20), it cannot be determined experimentally. An infinite number of experimental points, or extraordinary good luck would show it directly in the data; otherwise it is derived statistically.

6/2/2 Not necessarily. For nongenetic effects very little additional information appears after the first 3 or 4 months of testing.

6/5/2 Mutations are most easily visualized as physical changes because of the nature of the molecule and the nature of the change. (deletion of single or multiple bases, insertions, broken chromosomes etc.) However, almost all interactions are chemical reactions that result in adducts or result in bases being deleted from , or added to the sequence, or incorrect repair of damage. Why not just say "change"?

7a/Table 3-2. Amitrole fetotoxic NOEL as stated is inconsistent with both p7, bottom and page 8 top.

I would suggest rounding such numbers as the atrazine LD 50. Numbers presented with this apparent precision give a false impression of the ability to make measurements in biology. Somewhere it is necessary to make certain that readers understand such numbers to be merely the creatures of arithmetic treatment of measurements that themselves have but modest precision. Perhaps it would be useful to include the standard deviation and an explanation that this is simply a number that has a high probability of really being within a certain range of values.

8/5 (asulam 2), last line. The descriptor, "greater than" is used frequently and should be explained as relating to the highest dose tested, and limited or no effect at that level.

9/2/3 Kidney effects are systemic. I don't recall kidney among the list of organ systems or functions to be considered separately from general systemic effects.

Speaking of that issue, I don't agree that neuro- and immunotoxicity should be set apart as non-systemic. Every chemical, however non-specific, will hit some site before it affects any other. That doesn't make the effect specific to that organ or function unless the response is clearly at rather low doses relative to onset in other organs, or the effect is so characteristic that it must be set apart. The neuromuscular effect of 2,4-D is probably specific enough to identify as such even though it is quite rare in humans and requires high doses. It is not quite the same as the myotonia seen in rodents.

9/2/5 This implies that 2,4-D is not a teratogen, which is not correct. Other species do show such effects; it does not matter that EPA may not consider the other studies definitive. There is such a mass of data that shows positive responses that they cannot be ignored. The fact that the dose response indicates that the prospect of effects on humans is very remote is a separate issue.

10/4/Last line. How does this NOEL of 2.5 mg/kg/day square with the fetotoxic NOEL, fifth line from bottom? In my view, the reproductive or fetotoxic NOEL ought to be used as the working NOEL when it is the lowest number, except when the species is clearly inappropriate. Much of the time the effect is a function of maternal rather than fetal effects, even when no maternal toxicity is detectable. Consequently it implies some kind of effect that may well occur in both males and females unless it is highly specific to the female reproductive or endocrine apparatus or sex differences in drug metabolism.

13/2 The inappropriate use of dog data to establish NOELs on triclopyr or any other organic acid is well described here. The data and explanation should be mentioned, and no estimate of risk should derive from these findings.

18/Table 3-4, 2,4-D. My scorecard on mutagenesis shows a much higher ratio of negative to positive. In any case, it is a very weak mutagen, and within your rules for this exercise it is appropriate to assume it to be mutagenic for this exercise.

- 20/5 The Donna et al work is so poor it should never have been published, and it ought not to be used.
- 23/5 The newer data on triclopyr is in the hands of EPA. Shouldn't it be incorporated? A 228-day dog study would probably not detect even a virulent carcinogen.
- 23/Bottom-24. I commented earlier that the idea of cancer potency should be discussed earlier. This material should be at the beginning of this overall discussion, perhaps in an introductory page or two discussing concepts employed in estimating carcinogenic effects and risks. The idea of the 95% upper bound needs special attention in such an explanation. There is enough detail about cancer and cancer studies in this portion of the document that there may be a need to discuss the criteria under which laboratory carcinogenesis studies are judged. This would be particularly pertinent in the cases of glyphosate and 2,4-D.
- 25/Fig. 3-2 This graph suggests that the range of likely human doses is far higher than realistic. In effect the graph, with no numbers, says that human exposures produce doses as high as one fifth as great as those which produce cancer in the lab. Even though it is not the intent of the figure, someone will conclude that it shows real information about the herbicides proposed for use.
- It is worth bearing in mind that the Ontario Ministry of Environment task force, which found that all available evidence was insufficient to identify 2,4-D as a carcinogen, included Crump. Presumably, by this time EPA will have had time to finish its assessment based on the newer data, which may necessitate change in this section.
- 28/3/2 I believe it is known that the petroleum distillate in question is kerosene, used as a diluent for ester formulations.
- 28/4/21 Overstating the risks associated with the herbicides does not make the risks of the inerts insignificant, because any hazard that might be involved is almost certainly of a different nature. The draft is correct in stating that only one inert is listed as requiring attention. The treatment of this issue in both volumes should be brought into alignment. I have commented on the inappropriate discussion in the main DEIS.

SECTION 4

- PP 7-12 This section and the dependent Attachment B have given me more frustration than any other part of the project. I have tried to work my way through one of these calculations just to see if the approach is logical, and I cannot make any progress. Whether I am obtuse or the discussion is badly organized, I do not know. My initial problem is identifying the source of the figure on the last line of page 9, of 70 pounds of 2,4-D applied on average per applicator. Overall I think the process of estimating these exposures could be laid out in a much more useable form, so even I, or a supervisor on a project could use the process to estimate exposure in some specific situation.

2/2/5 This is an incorrect statement unless the immediate environment is clearly defined as limited to surfaces from which absorption can take place. Material trapped on clothing away from the skin, bound to soil under foot, or adsorbed on plants in the general vicinity does not constitute exposure.

6/5/8-10 I just don't understand what this sentence means.

8/3/1 Should there be a section at the beginning of the chapter discussing the methods that underlie determinations such as this? How will the lay reader be told that for some chemicals everything that goes into the body emerges intact, in time to understand this kind of approach. It is described reasonably elsewhere, but I think it needs to be explained on page 2 under exposure and dose. This is another example of the problem of information distributed far and wide in these books.

8/7/#2 As I read this, it implies that the entire width of that right of way is treated, gravel and all.

9/1/#3 When using a standard 10% absorption factor, which is appropriate when lacking any information, it should be pointed out that skin absorption is largely governed by fat solubility, and as a consequence, for substances that are not quite lipophilic (fat-soluble) skin permeation should be very low, as is the case with picloram. It is doubtful that even the ester formulation of triclopyr moves at a rate approaching 10%. Data on human absorption of triclopyr now exists and I presume it has been made available.

According to the review on amitrole in USDA 1984, page Am 31, "in the absence of data to indicate otherwise," the absorption rate for amitrole is considered to be 10%. Which factor is used here?

A figure of 0.48% for picloram absorption is also used, with reference to Lavy, 1984. The 1987 publication of that work in Environ. Tox. Chem. 6:209-224 shows no such figure, and in fact, in the abstract the authors say "If equal dermal penetration of 2,4-D and picloram is assumed,--", indicating that they don't have a penetration rate. It is likely that an estimate can be made from that data and some assumptions, but no indication of that exercise is evident. Furthermore, the question of picloram dermal penetration through human skin has been studied in the laboratory, and published, and it is indeed very low. (Nolan et al, Tox. Appl. Pharm. 76:274, 1984) Why neither Lavy et al nor the authors of this document did not use that data is curious.

22/1 The time frame of this exposure should be specified. In the case of peas, what accounting is made of the loss of the pods and herbicide deposited thereon?

22/ In considering exposure of animals, I am willing to accept that an animal will move in such a way as to expose most of the body surface, even though drift is unidirectional. It is in error, however to make the same assumptions for access of surface deposition to the skin. Because of the enormous surface area of the hair, and its natural repellancy, a rather small fraction will reach the skin, where absorption is probably similar to that of the human skin.

It appears that an assumption is made that all herbicide contacted is acquired by an animal passing through treated vegetation. If it is assumed that removal of herbicides from vegetation is efficient, serious questions arise, for reasons stated in reference to page 24, paragraph 3.

22/4 The figures for acquisition by grooming are apparently assumptions, for which I can locate no basis. There will unquestionably be some removal by grooming, but the calculations for deposition, absorption and grooming should be integrated, and the basis stated, even if it is a desperate guess. Guesses and dart-throwing are not necessarily improper if no other approach is possible, but the method used must be stated clearly.

22/5/3 This is not the way the issue was discussed in the background statement. The assumption in that case was that 10 % of the dermal and oral exposure would be assimilated, and that it would be retained, which in itself is very conservative. That is different from 10% of the total dose. Both assumptions are too conservative, because skeletal tissue retains relatively little of such agents, while residues in liver and kidney are relatively high. Those latter differences are admittedly difficult to factor in. The background statement assumed consumption of meat for two days. In this paragraph we are not told how many days of consumption are assumed.

In this particular area of secondary dosage there are a number of inconsistencies with the introductory discussions in the background statement and they should be reconciled with this document, or differences should be explained. It is likely that the background statement will be considered as being associated with this document, because it was constructed as part of the general effort of which this DEIS is also a part. Some commonality should be established.

Among other things, deer in this DEIS have a surface to weight ratio of 0.039 sq M/kg, compared to 0.029 for the deer in the BG statement, which may be overfed. These numbers may be compared with the figure for a small human on page 26/3, of 0.016. The shape of a human is not different from that of a deer to make that much difference, unless the person is truly rotund.

24/3 The use of Popendorf's insecticide model is inappropriate for use with herbicides. Fungicides and insecticides are for the most part intended to remain on the surface of plants, to either contact or be consumed by the pest of concern without affecting the plant. They are therefore much more available for skin deposition. They may also mobilize in moisture, such as the dew of early morning. In fact, Popendorf distinguishes between low, damp and high, dry foliage. If they were absorbed in appreciable quantities, the residues on edible products would also be greatly amplified, and would not be removed in processing or washing.

The Popendorf model relates specifically to organophosphate insecticides, and has some potential for application to others. However, an important part of the model includes levels of exposure that will cause a decrement in cholinesterase levels. In other words, it responds to exposures and consequent doses that have exceeded the NOEL, not just an arbitrary 100-fold safety factor! That is hardly consistent with the conventions established for this document.

The work of Gerry Stephenson's group at Guelph shows empirically that once 2,4-D is applied, it is very difficult to remove. (Thompson et al, Pestic. Sci. 15:353, 1984) Although data for most other herbicides is not available, similar behavior should be expected for those other than desiccants,, otherwise the herbicide would be ineffective. Metabolically active herbicides cannot function without entering the plant, and as a practical matter, if it does not either bind tightly to surfaces or enter the foliage, it will wash off and be ineffective. There should at least be discussion of this behavior in the discussions of mitigating factors.

27/2/9 Metabolism has nothing to do with exposure.

28/Table 4-9 I think logic ought to be allowed to prevail here. By day 30, no berry picker will go near an area that has been sprayed, let alone pick the berries. A hiker will shun the area as well, since the values sought will have been depleted.

SECTION 5

1/2/6 The ADI and MOS are both based on quality of data and on the NOEL. The only real difference is that with the ADI an outsider or an agency is incorporating some kind of judgement, and telling us what is acceptable. When using an MOS we are simply informed of the difference between field dose and NOEL.

1/2/9,10 This passage is confusing. There is a world of difference between the highest NOEL and the lowest NOEL. In the following sentence, in reference to non-carcinogenic effects, this could be read to mean the other effects caused by a carcinogen.

2/1/16 Risk is misused here and in paragraph 4 on this page. An MOS does not define risk; there is no practical difference between an MOS of 1000 and one of 10,000, or for that matter, 100. The MOS does not predict an effect or probability of effect. No one knows what an MOS of 50 means, but because we do not want to find out, we use an MOS of 100 or 1000 as a standard.

2/3/6-8 This sentence is curious, as are the tables that relate exposure level to LD 50. We are not properly interested in fatal effects, and I recall no place in this document where risk of fatality is discussed, the tables notwithstanding.

3/3/2 Some of these chemicals are listed as questionable carcinogens elsewhere. Use of "positive" needs explanation.

5/7 In judging the residues in deer and secondary exposures therefrom, it would be useful to also consider the data developed by Dow in experiments with goats given triclopyr. Very roughly, about 1% of a dose of 0.2 mg/kg remained in tissues at the end of 10 days, assuming conservatively that all tissues in which no triclopyr could be detected, contained levels at the detection limit of about 0.003 ppm. It should be expected that 2,4-D would behave similarly. These data are part of the proprietary package, but were provided to a citizen group through an FOI action in Idaho several years ago. They should surely be available to USFS.

- 6/1 This is a very important statement, because it brings reality into the analysis. As discussed elsewhere, this concept should be prominently stated early in the document. I would like to see it in bold type or with other kind of emphasis, because the calculations are really intended to see how bad it could get if each possible adverse condition could be met. Some of the statements later in the page, taken alone, can imply that there is a realistic prospect that the worst case depicted can occur, and that residents will be subjected to exposures that will lead to low margins of safety and that they may suffer acute toxic effects.
- 6/2/6 I think the use of amitrole needs a careful examination. As a carcinogen it is really of no consequence in the context of use in forestry; I agree with EPA that it is almost certainly a non-genotoxic agent. The effects on thyroid function are not trivial, however. Measurements in humans show that careless and poorly clothed applicators using amitrole will experience deficits in thyroid function within a few days of first exposure. These are reversible effects early on before compensation begins, and a single exposure probably does not matter. However, an individual with thyroid dysfunction or other metabolic insufficiency could be at risk of acute illness. If the chemical is sufficiently useful, special training and practices will eliminate the hazard. Amitrole represents an odd situation in which home users may be at less risk than people working with it regularly.
- 6/2/11 I question strongly that dilute 2,4-D in chronic exposure will result in neuromuscular disease. The cases of peripheral neuropathy on record have almost exclusively resulted from exposure to concentrated material in a single large dose. I am aware of assertions that dilute material under field conditions has caused such effects, but no reliable documentation has been produced. The excretory clearance of 2,4-D is sufficiently rapid that only a massive dose can produce the target loading necessary.
- 6/2/13 As the document points out later, the calculations on triclopyr are based on dog data, which is acknowledged several times as not being representative. The human renal excretion mechanism for organic acids is very efficient, and the assessment should be based on different numbers than those arising from dog work. However, this demonstration may properly lead to some mention of the consequence of heavy exposure of people with renal or hepatic disease. for most or all of the compounds.
- 6/3/12 Why does weight loss relate to potential stomach problems? Weight loss and similar indices are entirely non-specific, unless a particular condition is identified that might reasonably lead to the weight loss. Is a metabolic deficit involved? Is GI absorption compromised? Appetite failure can result from anything from CNS depression to peripheral pain.
- 7/3/8 I think an essential fact has been neglected here. A bushel of berries frozen and eaten every day without washing will not produce a cumulation of dosage, with illness. The statement implies ("might feel quite ill") that there is a good chance that sickness will occur. That conclusion cannot be supported. If I recall, the daily dose of 2,4-D in this situation would be 0.048 mg/kg. If this amount was cumulative, the total dose would be 30×0.048 or 1.44 mg/kg, which exceeds the chronic daily NOEL, although not very far. If parallelism between experimental animals and humans is assumed, that amount in a single dose may or may not have some effect; it is not likely, given that the NOEL is set from a chronic daily dose.

If we assume for the moment uniform distribution in the body and a disposition half-time of 15 hours, at steady state the concentration in the body will be about 1.5 times the maximum concentration on day one, and that maximum would be achieved by about day four or five. Because ingestion probably takes place once a day, the tissue concentrations on a whole body average basis would vary from about 0.025 mg/kg prior to eating to about 0.075 mg/kg (ppm) immediately after consumption, assuming instantaneous distribution. For the sake of this argument this concentration may be likened to a daily maximum point dose of 0.075 mg/kg, which is also far enough below the chronic NOEL that an adverse response would be unlikely.

The same mechanics have to be applied to game animals as well. For fish the BCF assumptions accomplish the same end.

13/Bottom paragraph. The identity of combustion products of triclopyr are generally predictable on the basis of the analysis I did for Bonneville Power a few years ago, although no proportions or quantities can be derived from that hypothetical approach. I would suggest consulting Bush et al, Arch. Envir. Contam. Toxicol 16, 333, 1987, for more specific data. Most of the authors are USFS scientists. The paper deals with 2,4-D, dicamba, picloram, triclopyr and dichlorprop on firewood.

14/4/1 Remove "the" There are many other PAHs that are probably carcinogenic, but there isn't sufficient data to analyze. The added risk from those compounds is probably not significant. Understanding of environmental behavior of PAH is not complete, but it is likely that their impact is less than simple dilution calculations would imply.

Work is in progress to estimate risks associated with aldehydes, carboxylic acids and acrolein in forestry smoke. Carcinogenic risk is probably negligible, but there is a very real potential for acute respiratory effects from this family of products.

16/4/8 I think this statement is too strong. It is likely that this level of exposure has been experienced many times, given the casual attitude of some workers, but the reports of impact are not there. It is better to describe it as having an MOS that is too small, and therefore requires tighter discipline and perhaps a different local decision process.

18/2 This is an important paragraph and ought to be emphasized in some manner. What relation is there between work protocols and worker instructions, and the EIS?

18/4 It seems highly unlikely that anyone would drink this water. Self mitigating incidents should be given their due. Undrinkable water and defoliated berry patches are examples.

21/4 No study can show conclusively that a chemical is not carcinogenic.

In the discussion of synergism, which seems quite reasonable, it should be emphasized that these kinds of responses are not a result of chemical interactions between chemicals, but are separate physiological responses in the body that may add or subtract. They will be expected to be threshold dependent as well.

32/5/1 The word "potential" ought to precede risk.

GLOSSARY

I have discussed the glossaries under Part A of this review.

ATTACHMENT A

1/Amitrole. Is this material lifted directly from EPA documents, or has EPA (1985a) been further summarized?

Paragraph 2, line 4 gives a dose range but no dose response. A minor point, but the statement suggests effects at 0.1 ppm and limited cell toxicity at 100 ppm, which is interesting, even though mutagenicity data cannot be incorporated into a quantitative risk estimate.

3/7 As noted earlier, it doesn't take a prolonged period for sloppy applicators of amitrole to demonstrate thyrotoxic effects. The description of the sequence of events is well stated, however.

5/ Atrazine study results, para 2. The dose response should be included here. This has been a rather surprising finding and should be detailed a bit better.

6/ Review on 2,4-D ought to be updated. The recent report by an expert committee of the Ontario Ministry of Environment will be found to be very useful.

ATTACHMENT B

Table B-3 and B-4. I have already mentioned that I became lost in these calculations. A specific problem relates to some data in Section 4, page 11. If I understand correctly, in the small plot there are two operators working 6 hours at the low rate stated in table 4-3. In the large plot there are 14 operators working 9 hours at the high rate. In addition, in section 4, p 11, there is a daily dose apparently derived from Lavy, et al, that is 2.5 times higher for the workers in the large plot. When these factors are summed, the difference between the routine-realistic and routine-worst-case situations follows. The high dose, as used for the routine-worst case, is the 95 percentile dose, stated to have been derived from Lavy et al. In the Lavy et al paper of 1987 in Env. Tox. and Chem. 6:409, which is the open publication of the Lavy et al 1984 reference, there is no total excretion for any worker that reaches a dose of 0.1895 mg/kg.

APPENDIX H

SECTION 1

None of the references in the text of this section are in the bibliography. In trying to locate certain of these references that I did not have, I was unable to find any reference by Lave and Omenn, or by Omenn and Lave. I would appreciate this information.

As I read through this section it appeared to me that it was brought from some other source and inserted into the document. Neither the writing style nor the evident philosophy seem quite consistent with the rest of the document.

Paragraphs 1 and 3 of page H-1 address the same issue in slightly different ways. Differences in scientific judgements and differences in policy choices appear to be linked in this statement, and indeed they are in an ideal world, up to the point where the society makes a decision to proceed beyond the scientific and make policy on the basis of other factors. This is appropriate, unless policy based on other considerations is declared to be based on scientific evidence. The policies driven by such a document as this are peculiarly vulnerable to such misassignment.

In paragraph 3, possibly not intentionally, this issue is addressed in the ambition to emphasize quality of information as well as content. That is a truly formidable challenge, because it requires a formal assessment of not only the literature, but the people who interpret it. In paragraph 2, the need for human judgement in bridging uncertainty is stressed. Again, to do this requires courage and a considerable incurred risk, even though criticism and appraisal is the basis for all scientific progress. We can never have complete knowledge, and we have no choice but to rely on judgement. The questions here, of course, are whose judgement is used, how are the qualifications and competence of, in this case, the scientists, determined, and then what process of scientific consensus has been employed, both in preparing the document and in evaluating such comments as I am preparing.

In practical terms this ideal of qualifying all scientific input may not be reachable, but it is approachable. Is USFS prepared to obtain independent judgement about the competence of the scientists who submit interpretations of the scientific data relating to risks associated with herbicide use? Even more important is the assurance of competence of scientists who opine about what may exist in the places where data do not exist. Would the Forest Service respond to such assessments if it obtained them?

The first paragraph quite correctly acknowledges that the "debate" has not made room for these questions. Perhaps the reason lies in the issue of perceived health risks, mentioned in paragraph 2. The vernacular has over time defined "perceived risk" as that derived from belief not necessarily based on fact. Whether fair or not, that meaning is established. Until the nature and background of perception is also explored, the earlier problems discussed in this section will not be comfortably dealt with.

It will be difficult for USFS to discuss the various bases for perception without making this a political document, but it should make a conscious effort to either do so or not. In either case it should then decide whether (and how) to incorporate perception in the decision package.

1/7/6,7 That a ten-million-fold difference between potencies of chemicals exists makes perfect sense, and is really immaterial. I am quite familiar with the paper by Gold et al, 1984. In contrast to the impression conveyed here, when I first looked at it I was struck by the remarkable concordance of dosage in the various assays on specific chemicals, not the differences suggested in this discussion. The statement about 1000-fold differences in potency observed among tests of the same chemical sent me back to the work to find such variations, and I found a few where extrapolations suggested differences of 100-fold, and they were not all that frequent. They also were usually for different tumor sites within an experiment, which also makes sense. I would suggest that the work was either misunderstood or misread. To be sure, it is a cumbersome paper, with a vast amount of information

crammed into a small space. It is, however, tabulated in such a way that once symbols are understood, doses and ranges are quite visible, both graphically and numerically.

The consistency of data is all the more remarkable when it is remembered that all of the data in the paper was generated in 1981 or earlier, with the exception of a few studies that were in progress and available in 1981. The good laboratory practices regulations that now provide much greater assurance of proper work were barely in place at that time.

2/2 This statement appears to imply a high level of unreliability in cancer assays. If that contention is supportable, it ought to be well documented because of its importance to the validity of this EIS. How many such instances were there in this great mass of data presented by Gold et al, or from other sources? What was the nature of the deficiencies, when was the work done and by whom? Even the nation of origin might be important.

2/4 I am very curious about the idea of a "null hypothesis" in cancer testing and other registration studies. This concept may well be employed in developing a research protocol, but testing for toxicity is not research except in the broadest sense. No toxicity "test" is valid until a maximum tolerated dose is achieved, which means that some kind of positive response occurs. An exception might be a case where a chemical is so benign that the dose rate is a large fraction of the diet. The question of what evidence is sought is considered only in the design of the study, such that cancer, terata or other adverse end points will be seen if the chemical can produce them, within the statistical limits of the assay.

Starting with a hypothesis that cancer will not be seen would bias the protocol and render it invalid. The bias that is introduced is actually just the opposite, with selection of cancer prone strains of animals. There is no null hypothesis in the pathological investigation of the animals; the pathologist usually has no idea which animals are treated and which are not. Tumors, precancerous lesions or other pathology are found or they are not. If they are not, statistical treatment provides some idea of the reliability of the negative data. Does it tell us that zero really means somewhere between zero and five percent or zero and ten percent, or are the numbers more precise? Does a positive finding fall within the boundaries of a chance occurrence, given the natural history of the particular tumor in concurrent and historical controls? Those and other questions are answered after the work is complete.

Furthermore, carcinogenicity data do not exist in isolation, to stand or fall on their own merits alone. Does the tumor progression fit accepted criteria, and what preneoplastic changes are seen? What is the pharmacokinetic profile for the chemical? What is its basic chemistry? What is its genetic toxicity? In fact the paper by Clayson and Krewski referenced on page H-5 speaks to these and other qualifying factors. (see comments below)

These pages convey quite strongly the belief that the data generated for the purpose of defining the toxicity of the chemicals considered in this EIS are of little value. This is curious thought, given the number of decimals used in the tables of Appendix C.

4/4 This is a polemical statement.

4/Interpreting False Negatives. The issue of social costs related to false negatives is of great importance. Because this document relates to use of specific chemicals, it seems reasonable to expect that once this issue is raised, there will follow a discussion of examples among the chemicals in which registration is based upon minimal and inadequate tests. With that should be some rationale for either utilizing or rejecting the experimental data generated in tests that do not meet EPA criteria. No such discussion is present. It would also be useful to know which chemicals were rejected before the draft stage for this reason. If the social costs of false negatives are an issue in this program it seems sensible to discuss the science and practices that prevent those costs in the program. We are now led back to the first three paragraphs of the section, however, because both perception and knowledge play roles in social willingness to place some level of confidence in the ability of the system to provide reasonable protection.

I have already pointed out that Clayson and Krewski have discussed the various characteristics of a chemical that contribute to the knowledge base used to judge carcinogenicity. In view of the paper to make the points about the social costs of false negatives, it would have been instructive to quote from the other considerations they developed. Their belief that the current protocols for carcinogenesis assays "represent a reasonable convention for the minimum criteria of a negative result", is very important in the light of the implications shed by the earlier extracts from their paper.

This is all not brought forward to show that we have perfect system. It is not, but it is pretty good. That these articles have been selectively quoted as they have been suggests to me that the section has been written with purposes other than information and instruction. The Clayson and Krewski article in fact is an excellent description of the soft spots we still have in this overall "pretty good" system. Such problems as thresholds and the limits in our ability to be certain that a "nongenotoxic" chemical is indeed just that, and uncertainties about the genotoxicity of supposed direct carcinogens are discussed quite lucidly.

These various points about the nature of scientific data can be very instructive to the general public, for whom all of these problems are just a black box. If they are to be discussed, it should be done with an objective of instruction, preferably by a good statistician with toxicological experience accustomed to teaching from the beginning. I believe there is an obligation to educate the public through this document.

6/2/4 If this means that pesticides may react chemically with each other, the likelihood is vanishingly small in the environmental context and not even to be expected in the tank. Physiological interactions (two chemicals producing similar responses in the body, which influence one another) are possible but are clearly dose and threshold dependent. This paragraph should be expanded to discuss this issue properly; as it stands it raises spectres that have no stated relation to the chemicals dealt with here.

- 6/6/2 Again, the data already reviewed in this DEIS should be brought together to support this statement. Simply saying that the herbicides proposed for use have not been adequately tested is not sufficient. See comments related to Appendix F Section 1, page 10/1. addressed by this EIS.

The general public, for whom this exercise is most important, does not know what a data gap is, and when it is significant and when it is not.

- 7/2 The reason for this statement is unclear. The inerts associated with all of the products discussed in this EIS are known. I believe the only listed inert of concern is kerosene, which has, in fact, not been fully examined, although there is a great deal known about petroleum distillates collectively. Unfortunately there is just enough difference among them that a direct transfer of data is not appropriate. The inert substances that are of direct concern to this DEIS are the ones that should be discussed, with some substantive description of what is known and not known about them.

I expect that the community would be a little less alarmed about kerosene than about formaldehyde or cresol or dimethyl hydrazine, which are on the list of inerts of concern to EPA, but are not found in the formulations identified in this DEIS. I would agree that the public is not sufficiently concerned about such common materials as these light oils and solvents, but in the context of use as pesticide solvents, with proper work practices, they do not represent a significant hazard.

This paragraph raises an alarm that is misplaced with respect to the chemicals proposed in this DEIS. See also comments related to Appendix D, Section 3, 28/3-4

SECTION 2

- 10/1/10 There is an enormous amount of data on glyphosate and triclopyr. If they are considered inadequate, the reasons ought to be stated. See also comments related to Appendix D, Section 1, page 10/1 and Appendix H, Section 1, page 6/6/2.

- 10/2/5 Something must be done to reach information that has appeared since the reviews relied upon for this EIS appeared. A comprehensive search of the open literature is not beyond the scope of this project, particularly with a major university contracted to assist in development. The cost and time needed for a proper search and analysis are miniscule compared to the costs of this document and the contracts associated with it. I think that industrial test data are more available for some products than is often believed. The necessary negotiations with industry do not compromise anyone, and contrary to some opinion, the data are not fiddled.

Triclopyr is a case in point. This document does not include the latest cancer studies. It is interesting also that some citizen groups have acquired registration data through FOI that is not in the hands of USFS.

SECTION 3

- 116/2/2 100 mg/kg is not a low dose.

- 116/4 Include in non-specific evidence of immunotoxicity any differential in colony infectious disease between treated and untreated animals.
- 116/5 In a way this is a non sequitur in that there are many other reasons to keep exposures much, much lower than 100 mg/kg.
- 116/6/5 Repeated and survivable exposures at 200 and 500 mg/kg? That would make for a very durable rat.
- 116/6/6 Should be 2,4-dichlorophenol, which is also a starting material, and in older 2,4-D, a contaminant, at less than about 0.3%. It is further degraded by the same systems in the environment that produce it from 2,4-D, and it is difficult to find except in a closed system.
- 116/6/7,8 30 ppm is a concentration, not a dose. When discussing exposure to 2,4-DCP it is necessary to refer back to exposure of 2,4-D, either in terms of product contamination or occurrence in the environment, because in the context of herbicide use, this is the only possible means of contact. (see above)
- 117/Asulam. Why is vomiting and anorexia necessarily a neurological manifestation?
- 117/Atrazine. If the effect of Toxurazine is important here it ought to be also under amitrole. At such levels, in a mixture like that, the information has little meaning.
- 118/2,4-D Brain lesions. In both cases there is no way to know the source of lesions. The subject in Nielsen et al convulsed, which in itself could have caused the damage. In Dudley and Thapar, the subject drank the 2,4-D in a pint of kerosene, which will cause such lesions. These findings say neither yes nor no about the ability of 2,4-D to cause brain lesions.
- 119/3 It might be useful to note that in the Desi paper, animals that didn't die recovered completely in about an hour, in terms of the measurements they were making.
- 120/1 See earlier discussion of 2,4-Dichlorophenol. If discussing 2,4-DCP as a product of 2,4-D degradation, it is necessary to also discuss its fate in the environment and in the animal. It is probably impossible to achieve a significant concentration of DCP in a 2,4-D exposure.

It is necessary to know whether the material was introduced in diet, on skin, or what? For the purpose of this document, the context of exposure of the parent product must be explained if the issue has any importance.

- 122/Tebuthiuron, line 3. This is probably RIGHTING reflex.

EDITORIAL COMMENTS

These comments do not encompass the entire document, or even all of the points I saw as deserving editorial attention. The items I have noted do suggest that the entire document(s) should receive careful scrutiny by someone familiar with technical usage. The issue of public readability must not be ignored.

SECTION 1

- 3/2 I question whether the sentence describing risk analysis could survive the readability test.
- 11/3 Organization of the supplement. This paragraph ought to be at the beginning so it can be found and followed.

SECTION 2

- 6/2 There are no aerial silviculture projects. There are aerial applications of pesticides for silvicultural purposes, etc.

SECTION 3

- 4/2 The paragraph refers to table 3-1, relating LD50 and NOEL, and there is no mention of NOEL in the table.
- 5a Threshold and no-observed effect dose or level ought to be in the glossary. It may be in some other glossary, but not where I looked.
- 6/2/3 I don't think this is clear. I would suggest redrafting the whole paragraph.
- 8/1 Isn't this paragraph part of the discussion on page 7 in the form of a quote or lift from an EPA document? If so, indent.
- 9/2/4 Why not say that at the highest dose for rabbits of 7.5 mg/kg/day and for rats of 7.92 mg/kg/day, there were no etc. The sentence is a little confusing as written.
- 11/1/5 Awaiting, etc. Is this a work note that evaded detection?
- 20/1/3 Does this mean no oncogenic effects, or other effects than oncogenic? Line 8, leave out word "on."
- 20/5 The NNA discussion should be in a separate paragraph.
- 26/5/9 2,4-D cancer potency. The sentence, EPA has studied, etc, is apparently misworded.
- 28/4 This paragraph badly needs slicing up into pieces.
- 29/3 Specify the difference between diesel fuel and diesel oil, if there is one. This is a bit confusing.
- 30/1-3 There is considerable redundancy here and on the prior page. These two pages could be reorganized and consolidated.

SECTION 4

- 13/3/12 The sentence, "Only dermal field etc.----" doesn't make sense.
- 24/1/7-11 Rewrite.

- 24/ Sentence in middle of page, "However, etc.---" peculiar wording that doesn't make sense.
- 27a/ By the time we have arrived at this point we have forgotten what the routine-worst case is. On tables such as this the exposure should be restated, so readers do not have to thrash around looking for the numbers.
- 27b This material ought to be incorporated with the material on page 24. I think it would come out somewhat more cohesively.

SECTION 5

- 2/1/11 Should probably read laboratory-determined NOEL. As used here, "level" is not associated with a parameter, such as birth defects, skin irritation or nystagmus.
- 12/Middle, sentence beginning, "The expected dose--- has a 10 E6 error.
Should be 2.2 x 10 E-3.
- 19/Table 5-8. Is this for concentrated material? There are some extraneous words in the title.
- 28a/Table 5-15. Second column should be time to accumulate, etc.
- 29/7/6 This statement is not consistent with other statements in the document.

SECTION 6

- 2/ Dost middle initial is N. Klaassen, in Doull et al is misspelled. (The third Edition has been out for some time) Those are just two I happened to see.

ATTACHMENT A

- 9/3 Is this the Gulf South Research Institute?

ATTACHMENT B

The tables in this section and elsewhere carry numbers out to far more decimals than can have any meaning. My analogy to this is comparing risk assessment to measurement of the volume of a room by measuring one dimension with great precision, measuring the other two dimensions with an eyeball estimate, and carrying the multiplication to four decimals.

APPENDIX H

SECTION 2

There is inconsistency in organization, with pages in this appendix continuous and others repaged section by section.

- 9/3/9 The wording here suggests that this is a transplant from the old EIS.

FND:mcc

Review of Health Impacts Sections of Draft Environmental Impact Statement

PART B: REVIEW OF DEIS, MAIN DOCUMENT

Subjects of these comments may be located within major headings by page/paragraph/line, unless otherwise noted. A partial paragraph at the top of a page is paragraph one.

SUMMARY

7/Fig. S-2 Alternative D. In any system, last options or last resorts means that an alternative has not been included in the planning on the same basis as all other options; the implication is that other options have failed. In that event, the last option is also doomed to fail. At this early stage, application of such wording to any option means that some a priori judgement has been applied before evaluation has been completed.

10,11/ Is "receiving no treatment" management?

10,11/ These numbers say that exposures of workers and the public are essentially the same within every alternative. It is questionable in the extreme that risk to workers and the public are similar, and practically impossible that they can be identical, as they are in two cases. It may be that risk index has a relation to other factors to which I haven't been introduced. Unfortunately, I have no idea what the risk index is; I can't find a definition and like many critical terms it is not in the glossary.

The definition of these terms is sufficiently vague that risk to workers (first line), which includes three components that can contribute to injury accidents, is not distinct from injury accidents. The last line, then, is a large number that can be read as a fraction of the smaller number above it.

12,13/ It is very important that perception is recognized as important, as it has been here. However, if it is a factor in decision making, it should be defined and analyzed just as are other factors, such as safety, efficacy, yield and so on. I can find no analysis of perception nor is there a method of using it in the decision process. Perception applies to all aspects of vegetation management, not just risk.

15/5/1 This statement deals with human risk rather than environmental and is out of place.

It is essential to incorporate risks associated with sanitation problems resulting from large numbers of workers in the woods, as well as mitigation of those problems.

16/2/Bullet 1. "Downstream" should be defined with respect to both distance and surface vs ground water.

16/2/Bullet 3. I do not believe enough attention has been given to the respective hazards of hauling mixes vs concentrate. It is better to carry concentrate and water separately, with mixing at the landing or work staging site. A pickup

carrying sealed five-gallon cans of concentrate could capsize without spilling a drop; a tanker almost certainly will spill if it upsets. It is far easier and safer to deal with 5 gallons of concentrate than 100 gallons of mix containing the same amount of chemical. Also, the probability of a tanker being wrecked is probably greater than that for a smaller vehicle. Have there been differential probability estimates for accidents involving light and heavy vehicles? When an accident occurs on a road in steep terrain there is little chance to contain the spill. Landings are flat, and spilled material will remain in place for management and degradation. I don't know what is required in the EIS for spill management, but it would be prudent to consider the availability of spill management equipment on batch vehicles.

16/3/Bullet 7. MSDSs are by and large useless to a worker, and not much good to a professional, as they are currently written. They fulfill a legal requirement. They are being improved, but I would suggest producing concise understandable summaries independently of MSDSs, for the purpose of informing and educating workers.

18/ Here we are again at the question of comparable health risk for workers and the public. What is the basis for these statements? I am particularly entranced by lines 2 to 5 of paragraph 2. Such similarity, and identical risks in two completely unlike cases suggests to me that there is a magic number included somewhere, or a definition of risk that I have not yet encountered.

20/ The box in figure S-6 speaks to benefits. There are no benefits in this figure. The box is stated to relate to figure II-10 (2-10), which is many pages ahead and which we have already seen as S-3. Note that the box in S-7 refers to S-3 and the box in S-6 refers to II-10 for the same data. This is a good example of the problem of mixing Arabic and Roman numerals, as discussed in the general comments.

Do we assume that "injury" in figure S-6 refers only to physical injury?

21/ This figure is set up with numbers that have no definition that I can find, and since there is no reference to the appendices, I am not going to search for it. This situation is common through out the documents, with information scattered so that it is very difficult to trace down relationships. I do not think the documents would pass the test of readability.

With no definition for the indices, a naive person would read this figure as saying that risks are greater than value, just because the numbers are larger. The graph would probably be as useful if the risk index is divided by 100. In fact there is no valid way to relate risk and benefit. R/B analysis can only be done in the mind of an individual, in the context of individual values.

22/2/1,2 What is the public health risk, real or calculated, in a system with 100% manual management? On page 25/3 there is stated to be no public risk.

23/Bullet 1. Include work in the forest.

CHAPTER 1

No comment

CHAPTER 2

12/ "Time for action." The "first clear sign of potentially significant damage" cannot by definition appear in the planning stage, because by all the other conventions in this document, planning takes place before harvest, to prevent problems. This is spoken to in line 5, but as written the paragraph is contradictory. The last sentence again implies response after a problem emerges.

14/Bottom. Define "ground." As written, backpack and ground are partially redundant.

15/Bullet 3. What is the basis for prohibiting burning of treated vegetation? No basis in safety considerations has been established for doing so. If this is a perception issue or an arbitrary management decision, so state, or bring forward real data to support the position.

There is nothing intrinsically wrong with making decisions arbitrarily or on the basis of perception. However, there is no provision in the decision-making matrix of this program for either arbitrary decisions or decisions based on perception of risk, or perception of any other factor.

24/3/4,5 I am appalled by failure to deal with the very real risks associated with exposure to non-herbicidal chemicals, or physical hazards such as noise. Some of these chemicals and their degradation products are potent carcinogens, with exposure levels much greater than those of similar compounds in wood smoke. In many cases exposures to these substances can be defined at least as well as some of the quite speculative exposure scenarios set up for herbicides. Their intrinsic toxicities and the risks associated with them may be greater than those of herbicides. Mitigation measures for these hazards must also be included.

25/ bottom. Is there true proportionality of risk associated with areas treated at the unit level? Does a 100-acre burn pose a proportionally greater risk than a 60-acre burn?

In the same paragraph: There is danger in using disputed assumptions as an excuse for not calculating the number of people that might be affected, because there are surely many other disputed assumptions that are employed for other purposes throughout the EIS.

Perhaps it would be best to rewrite the last two paragraphs on the page, because there can be some confusion. The statement is made that there are risks to workers and the public from herbicides and smoke. Careful reading of the next sentence seems to indicate that risks within a method can be related to acreage treated, but it is not difficult to see a meaning which indicates that the two methods can be compared. Later it is acknowledged that there is no evidence that one method is more risky than the other.

- 26/1/4 Not necessarily. Conditions ought to be defined. In many cases, increased worker risk is incurred in exchange for no real decrease in public risk.
- 26/2/ Again we have this confusing comparison of worker and public risk. Here it says they cannot be compared, and figure 11-5 say they are the same. I cannot understand what is being said here!
- 37/Middle. What is the source of these figures on particulates ? I have no way of identifying the source of data.
- 38/Bottom line. What in the world does "high risk" herbicides mean? "High risk" has a definite connotation that is not justified by any analysis in this document. Even amitrole is not a "high risk" chemical.
- 82/ Will there be research in the monitoring program to learn the effectiveness of buffers? Downstream monitoring alone is not sufficient. There should be applied research built into the vegetation management program to provide data where assumptions are now used. It is necessary to learn whether the conditions now thought to be extremely conservative are conservative enough, or too much so.
- 82/Bottom. For aircraft operations, there should be additional rules prohibiting turns or patterns over residences or sensitive sites.
- 83/#10 See earlier comments on transport of mixed herbicide.
- 84/#19 See comments re page 16/3.

CHAPTER 3

- 47/Bottom. These numbers are not consistent with the Table III-7 to which they are related. It appears to me that there are a minimum of 6300 person-days in silviculture spraying.

CHAPTER 4

- 9/2/6,7 I think USFS is in a position where it will have to obtain a definition of scientific credibility. It may require a task force of the stature of an Academy committee to look at "scientific opinions" to see which are supportable by the data and accepted standards of reasoning. This relates to earlier comments about a matrix that would permit decision making on a basis that is deliberately non-scientific, with the provision that the factors entering into the decision must be stated.
- Page 11/4/9 The distinction between policy and science is noted; there is often a tendency to mingle the two. In this case there is policy already established; will science supplement or contradict? To what extent does Forest Service policy permit science to guide policy?
- 10/Top. Correct as of the moment, except that the methods to obtain the needed information are probably available, but the cost of doing the work is staggering. There are certain pollutants that have a high probability of being important, toward which the available resources should be aimed.

20/Bottom. This is no mystery. All reactions go slower as temperature is lowered.

21/1 I know of no herbicides that have an adverse effect on soil microbes at normal application rates.

40/7/5 The issue under discussion here is reactions between chemicals after release from the combustion process. Such reactions are most likely to occur either in the zones close to the fire, or later after dilution, usually as a result of photochemical activity. In both situations the energy necessary to react otherwise reluctant participants is available. The net products of both combustion and post combustion reactions are accounted for in atmospheric sampling, depending on the specificity of sampling. For many products, it is not clear when formation takes place.

This kind of interaction is not synergism. Synergism relates to chemical or other effects, usually in biological systems. It does not refer to reactions of chemicals or their products in the environment. The probability of nitro- and keto-polynuclear aromatics in rural fire areas and urban atmospheres is of interest in this regard.

41/3 For many products of combustion the question of adsorption on, or solution in, particulates is still unresolved. Basic physical chemistry would suggest that many or most combustion products will trap on or in particulates. The secondary question is about the fate of products after association with particulates, and there is evidence of both prolonged and shortened persistence, even for the same groups of products. The question is still quite open.

46/Bottom. What were the solutions? Was this concentrated material?

48/Top. Is there data indicating a firm relation between acreage and risk, except as the number of acres implies a greater number of projects? It should be expected that as the number of acres per project increases, the risk should fall, because of fewer mixing sites, fewer applicators, fewer trips for transport, etc.

91/2/3-5 How will this increased risk be justified as a trade off for a public exposure that is essentially zero away from sensitive areas? Remember that the probability of exposure of an applicator to an herbicide is to all intents and purposes, one. A large number of backpack applicators will each be certainly exposed to five times more material than a small number of workers in an aircraft application, to prevent a miniscule putative public exposure that has a small probability of occurring at all. This does not good numbers make.

91/4/3,4 As this is set up it is easy to get the impression that spraying is carried out with wind toward sensitive sites. Good practice forbids such application, or even application in dead air. I think that maximizing assumptions such as these need to be vigorously footnoted in the various tables and statements of risk, or they will be badly misunderstood. The highly conservative approach is very useful, but the points at which it bends reality must be clearly noted.

- 93/3/3 The word "toxin" is misapplied when referring to a chemical. Its general usage is in reference to substances of biological origin. Use "toxic substance", "toxicant," or some other suitable noun. And please don't use "toxic" as a noun.
- 95/1 Call a spade a spade, but put it in the past tense. The toxicity of dioxins characteristic of 2,4-D is very low, and they are present in very small quantities, when found at all.
- 95/Second from bottom. Chloracne is not a marker for exposure to 2,4-D. It is characteristic of exposure to some chlorodioxins, PCBs and a few other structures.
- 98/ Hardell references may not be consistently noted.
- 110/4/3 There needs to be a clear distinction drawn between cancer hazard and cancer risk. There is nothing in the glossary that assures this understanding.
- 111/1 Where is the notation of thyroid effects for amitrole?
- 112/2-4 I think the adjectives of high and moderate are seriously misused in this entire section. Usage relative to 2,4-D is an example. Moderately high toxicity to me means that there is a good chance that use or exposure will cause an observable adverse effect. That potential is not demonstrated by the analyses in the EIS. In the case of 2,4-D, moderate potential for neurotoxic effects in laboratory studies is derived from doses approaching the LD 50 in some cases, and the doses required for immunologic effects were also quite high. Acute neurological responses in humans are quite important, but they apparently also required heavy exposures. The only epidemiologic study that I recall showing neurological deficits at the level of occupational exposure was that of Singer. You may want to ask a neurologist for an evaluation of Singer's work.
- There is a contradiction in that 2,4-D is identified as having a low cancer risk in the first paragraph, and in the third paragraph, it carries a moderate cancer hazard. Both statements relate to animal data, and further confuse the risk/hazard definition. I think most toxicologists would not accept the argument that current data supports a "moderate" vs "low" carcinogenic potential.
- 114/3 How is glyphosate considered to have a higher cancer hazard than picloram? Look at NCI report 23. The work wasn't all that good, but aside from Reuber's opinion, it showed responses of a similar level to those of glyphosate at a high dose that was half that of the glyphosate work, which was associated with nonmalignant tumors of questionable statistical significance.
- 115/5 Triclopyr does not have high toxicity. If it is highly toxic it would cause adverse effects at low doses. It does not. I think there has been too much dependence on formulas and not enough attention to whether they make sense. Nothing in the paragraph in which this term appears justifies the use of "high toxicity" and in fact the descriptive terms indicate low toxicity. In addition, if this categorization has been based on dog experiments, it is inappropriate. There seems to be some failure in transference from Appendix D, section 3, page 13, where triclopyr is characterized as "slightly toxic". The concordance between the DEIS and Appendices and other sources will have to be carefully examined. I am sure I have not seen all inconsistencies.

116,117 This statement is misleading, because it gives an impression that all of these materials, and perhaps others beyond those examples, are found in formulations proposed in this EIS. Which of the several inerts mentioned in these paragraphs are in the herbicides considered in this document? Region Five was able to obtain the necessary information from EPA and learned that there is only one listed "inert," kerosene, in the herbicides they propose to use. To my recollection Region Five is proposing a similar list to that of Region Six.

Those inert ingredients pertinent to the program should be discussed fully, and there are aspects of kerosene toxicity that are worth noting, none of which are mentioned here. However, very little that is in these paragraphs on inerts has any utility in this document.

117/3 The word "toxins" is misused here.

117/6,7 What is the meaning of the sentence on particle binding in this context? That "interaction" if such it is, has no applicability to herbicides and associated materials.

It is true that there is no direct information on physiological interactions among herbicides, but there is an enormous body of research on drugs. The prospect of synergy in the context of uses described in this document is in practical terms zero. (The exception would be an applicator under medication for thyroid disease, working with amitrole.) The question is one commonly asked in the community, and it deserves a much better discussion than this.

118/1 Do contaminants represent a hazard with use of any of the chemicals mentioned in this document? If so, the matter should be detailed. If not, so it should be stated. This kind of statement has occurred over and over; it is not scientific, it is not informative and is probably inflammatory. I am not far from being persuaded that these usages are not accidental.

122/5 This should include the exposure assumptions and the selection of the BaP/particulate ratios. An estimate 10-fold lower is also valid.

125/3/3 There are two Peterson, 1983 references. Which is which?

FND:mcc

Synopsis of Peer Reviewers Comments on
the USDA Forest Service, Region 6 Draft EIS
on Vegetation Management

Three nationally known toxicologists, Dr. David Brusick, Dr. Edward Calabrese, and Dr. Richard Thomas were asked to review and comment on the risk assessment for human health in the Region 6 Draft EIS. They were requested to review the University of Washington's "qualitative" risk assessment (Appendix H) on the use of 16 herbicides for vegetative management in the USDA Forest Service Region 6 Draft EIS and the conclusions drawn from it in Chapter IV of the EIS for accuracy and appropriateness in the characterization of toxicity and risk. In particular, they were to indicate how well the University of Washington analysis characterizes the adequacy of the data base and what impact that has on the risk discussions, suggest better or more appropriate ways to characterize the risks of the vegetation management program, and indicate which risks, if any, are unacceptable and suggest mitigation procedures to reduce those risks.

In general, the peer reviewers found that the issues the University of Washington raised in their analysis, particularly variability in the quality of tests, lack of tests for certain endpoints on certain herbicides, and difficulty in extrapolation from animal models to humans were important considerations in risk assessment. The reviewers agreed, however, that despite the limitations of extrapolation in toxicology in general, and despite the limitations of the data on each of the 16 herbicides in particular, Government agencies must draw inferences about

human risk based on that information and spell out those risks for the decision maker, which the University of Washington qualitative risk assessment failed to do. Rather, the risk assessment served to some extent to confuse the picture of what inferences could be drawn about human health risks based on the available data (see Dr. Brusick's general comments).

As preparers of the quantitative risk analysis, LABAT-ANDERSON also wishes to make a number of suggestions for your consideration.

1. It is confusing to have two different risk analyses in the document that present such inconsistent discussions of the same chemicals. These inconsistencies need to be either corrected or explained outright. For example, the University of Washington uses a number of different NOELs for their qualitative analysis. We believe that the NOEL's used should be consistent throughout the document. The University of Washington also refers to the quality of available information in a way that is quite different from our own evaluation and in some cases from that of the U.S. Environmental Protection Agency. You need to clarify whether we were both working with the same data sets (I think this is a problem in some cases) or whether we were simply using different standards of evaluation. Our standard of evaluation is explained in the latest draft of BLM Western Oregon, which I understand that you have.

2. Also, we believe that there are a number of incorrect statements in the section on inert ingredients, interactions, and impurities. For example, EPA does regulate inert ingredients, although, thus far, they have not required extensive testing. EPA must certify that all pesticide

formulations, including inert ingredients, do not pose unreasonable risks to human health and the environment. Also, impurities would be present in technical grade material, except those caused by inert ingredients. Dioxin contamination of 2,4-D has been extensively studied by EPA and not found to be of concern. Please refer to our discussion in BLM Western Oregon for our latest thinking on the subject. Please note that the discussion in BLM's noxious weed EIS was extensively commented on by the Department of Justice and has withstood court challenge.

3. We think that it is confusing to mix the terminology of the adequacy of the toxicological data base with the risk of human health effects. See Tables IV-24 through IV-26. You may want to talk instead about the potential (high, medium, and low) of adverse health effects occurring.

4. We have extensively updated the hazard analysis in our appendix based on the Department of Justice's experience with BLM's noxious weed EIS and the most recently available data. We have also changed a number of NOEL's. We suggest that you replace the current appendix with the latest version that will be given to BLM after it is peer-reviewed.

5. You also asked us for ways of reducing risk. We suggest that the Forest Service may want to consider requiring protective clothing for workers beyond that which is required on the EPA label. Many of the spill scenarios project unacceptable risks to workers if they do not wash off.

COMMENTS OF DR. DAVID BRUSICK

October 1987, Draft EIS, Qualitative Risk Assessment

Appendix H

I. General Comments:

The concept of a qualitative risk assessment is troublesome in the sense that it confuses an already complex and confusing process of establishing levels expected toxic effects in humans following some degree of exposure to a chemical. Quantitative risk assessments are already burdened with assumptions and very "soft" numbers consequently alluding to the possibility that the risk assessment process can be accomplished without quantitation both gives the wrong impression of what can be accomplished and significantly dilutes the usefulness of quantitative efforts by avoiding the use of bounds on extrapolated toxicants.

The "Debate" correctly stated that uncertainty is one of the real barriers in dealing with risk; however, dealing with risk qualitatively does not eliminate this barrier.

The introduction cites the "lack of accurate measurement tools" as a source of difficulty in dealing with quantitative risk. I disagree to some degree. I believe the measuring devices used in collecting toxicity data are quite accurate and probably suitably sensitive. The real problems rest with the inability to understand the background variability of toxicity endpoints and to know how much of a deviation from this background level

represents a real risk. Both of these issues are quantitative and cannot be dealt with in qualitative terms.

Several of the discussions in H, Section 1 (false responses, power of the tests, etc.) address important issues in developing test data and are covered in almost every government document, book chapter, or review dealing with risk analysis. Consequently, this introduction doesn't add much to the document and it also doesn't develop a good case for doing qualitative risk assessments.

Section 1 also failed to discuss other important issues such as dose extrapolation. On page 5 of Section 1 the authors indicate that using the upper 95 percent confidence interval as an estimate of risk might be too conservative. In my opinion this is just what is needed to achieve a "worst case" estimate using data with the current degrees of uncertainty.

The following sections covering specific toxic endpoints were characterized as follows:

1. The authors set forth summaries primarily from secondary and/or tertiary sources.
2. The authors used the conclusions from their sources and did not attempt to integrate results or reach consensus interpretation concerning risk.
3. For the most part, these summaries were redundant with the LAI data compilations.

Ultimately, no risk assessment, qualitative or quantitative, was reached in Appendix H. I cannot understand how this Appendix will help clarify the risk from herbicide applications.

II. Relationship of the Contents of the Appendix to the text of the draft EIS:

There are issues critical to risk assessment which were discussed in the text of the EIS (Chapter 4) which were not addressed in Appendix H but should have been.

1. Dose response - especially the problems in extrapolating efforts found at high levels to low level intermittent environmental exposures.
2. Exposures - what constitutes an exposure and why is estimating exposures absolutely necessary when "risk" is discussed.

Inclusion of Appendix H does not, in my opinion, augment or clarify the LAI attempt to define a semiquantitative estimate of worst case risks.

Appendix H did not in any way improve the risk estimates calculated in Chapter 4 of the EIS nor did Appendix H suggest that some or all of the estimates are inappropriate.

III. How to better characterize the risks from the herbicides:

Chapter 4 and its underlying documentation in Appendix D were neither comprehensive nor entirely quantitative. Obviously, some data, such as that for mutagenicity, cannot be handled quantitatively. Mutagenicity data cannot because no models for risk assessment using short-term data (studies not conducted on the germ cells of animal models) are available and because no human epidemiology exists.

I would suggest the following:

1. Establish some consensus rules for when and how short-term mutagenicity data may be used in quantitative risk estimation for genetic effects or for cancer.
2. Select a method for integrating and interpreting the large mass of short-term test results. The method should be able to handle mixed responses and should yield a quantitative estimate of concern for hazard.
3. Adopt the concept of calculating a worst case cancer risk using an upper bound risk from the high dose 95 percent confidence limits.
4. Develop a method to normalize doses and exposures (acute vs. chronic) from animal experiments.

IV. Relative degrees of risk:

I believe that short of acute human lethality, which is not likely following exposures to herbicides, those risks which should be emphasized most are the nonreversible type (genetic, cancer, teratogenic, some neurological). These are also the most difficult to quantify and extrapolate. Possibly more space should be devoted to explanations of why nonreversible effects are more difficult to estimate.

Another distinction which is needed is that between probability risks (cancer) and idiosyncratic risks (allergic responses). Animal models are amenable to the former but of little or no value in determining the latter. Idiosyncratic risks probably account for a majority of the anecdotal "testimonial" evidence for toxicity in humans. Worst case assessments cannot protect these individuals and this fact should be acknowledged and discussed.

Received from Dr. David Brusick

2/11/88

The following comments are directed to the LAI quantitative risk assessment made for Forest Service and BLM:

Adequacy Ratings - consider the value of these. They are not defined carefully, they are subjective to a large extent and they do not lead to a better estimate of hazard. Where possible, I would suggest the use of degree of confidence that effect will be observed under "normal" or "worst case" exposures. You have done that for systematic and reproductive toxicity but not for cancer or genetic toxicity. I would suggest an attempt should be made to do such and make a large summary table.

Definition of Toxic Consequences - an understandable appreciation for the types (examples) of toxic responses that might result from genetic damage, immune toxicants and neurotoxicants is not provided in Chapter 4.

Exposure Frequency and Implications for Toxic Effects - a short discussion was provided on IV-92 of the Region VI document. I would recommend a more detailed review of this concept, especially for cancer, reproductive and genetic toxicity.

Cancer Initiation - on page IV 93 of the Region VI document it is stated that "it is widely believed that carcinogenicity can be initiated with a single hit." This is probably no longer true; however, for purposes of a conservative estimate of risk, the concept is applied.

Epidemiology Studies - I would recommend that you attempt to prepare a summary table (quantitative) from the cancer epidemiology discussions. This would make all of the text more understandable.

The following comments are directed to data presentation and use the Region VI EIS as an example. The comments are applicable to all LAI documents.

Table IV-10. under toxic effects it is not clear from some phrases what the toxicity might be. For example body weight - how should the reader interpret this statement as a toxic effect? The phases should be more descriptive.

Tables IV-20 and IV-21 - I would suggest that you add, if possible, an "Effects" column to the Reproductive section of Table 20 and, if possible, to the neurotoxicity and immunotoxicity sections of Table 21. What was the nature of toxicity reported?

Table IV-22 - a weight-of-evidence scheme should be used to produce a single descriptor for mutagenicity (i.e., is the compound mutagenic, non-mutagenic or indeterminate). Use of the A-, A+, M-, M+, etc. is of little value. There are several schemes that can now be used to generate such a description (i.e., Hart and Brusick, 1987). I would also strongly recommend that you modify the mutagenicity side of the table to the following:

Sample Table

Chemical	Data Quality ^a	Animal Mutagenicity		
		Effects ^b	Target Site ^c	LOEL (mg/kg/day)
Atrazine	M	+Clastogenicity	G	2100
	A	+Clastogenicity	S	1400
Simazine	A	+DNA Repair	S	35
2,4-D	M	-Clastogenicity	S	
Etc				

¹Quality associated with specific study(s)

^b+ or - plus type of DNA damage

^cG = germ cells

S = somatic cells

I would then make a separate table for in vitro and submammalian data indicating that these are not directly applicable to risk assessment for cancer or heritable mutation.

Table IV-23 - once Table 22 is changed to the above format, Mutation (animal) can be added to this table.

Tables IV-24-26 - why can't animal cancer and animal mutation data be added to these tables?

Herbicides of particular concern - pages IV-115 and 116.

In reviewing new data, I agree with the conclusions reached here and do not believe they have changed substantially during the past 24 months.

COMMENTS OF DR. EDWARD CALABRESE

Review of Appendix H

Human Health Risk Assessment (Qualitative)

Section 1 - Qualitative Risk Assessment

1. References given in the write up were not found anywhere in the document.

2. This section considers some of the many uncertainties that the toxicological community faces in the design and interpretation of studies especially with respect to human populations. Particular emphasis was directed to statistical considerations and the issue of false positive and false negative conclusions. Of particular importance to their interpretation was the contention that toxicological studies are designed with a considerably greater efficiency for detecting false positive findings while with a much lesser efficiency for false negative interpretations. Yet a false negative finding has been estimated by one of the authors to have considerably greater social cost than a false positive.

3. The section also emphasized other more biologically driven uncertainties in the extrapolative process such as interspecies differences in metabolism of xenobiotics, multiple chemical interactions, and the challenge of identifying and protecting groups considered at enhanced risk.

Perspective

The issues that the authors of Section 1 raised are genuine concerns. They are correct in their implication that those concerns are often considered to a very limited extent in many human risk assessments.

This information would be able to be put to more effective use if it were placed in a broader toxicological perspective in which one assessed the likelihood that predictive animal models and testing protocol would either under or over state potential human risk. Section 1 only presented a brief overview which tended to emphasize strongly the likelihood of understating concerns. Both possibilities exist in the current testing schemes and both need to be fairly addressed.

Section 2 - Data for Analysis of General Systemic Toxicity

Section 2 considers an evaluation of the herbicide chronic toxicity information data base which was used for the Draft EIS document. The authors of this section developed their own qualitative rating scheme for the overall data available and its applicability for use in deriving NOELs. The scheme involved a determination of when the data were inadequate (I) to make a determination, minimally adequate (M), and adequate (A). The authors then applied their rating scheme to the herbicides in question and made conclusions as to which herbicides were listed in any of their three ranks of I, M, or A. The authors made the tentative conclusion that for two agents (dicamba and diuron), the information cited in the draft EIS document were inadequate to judge the suitability for risk assessment.

Perspective

There is little question that the larger and sounder data base that exists the more confidence one will have in any risk assessment determinations that are derived from that data. The categorizing of the quality of the data into I, M, and A were reasonable although professionally subjective. The issues at stake here though are not entirely scientific. It involves the interface of scientific knowledge, scientific judgement and a need to act. The scientific process is fundamentally conservative, and at most, will allow the process of interpolation, not to speak of extrapolation. However, often decisions have to be made when even limited or no chronic data exist. Several research teams have been publishing possible extrapolative schemes for human reference doses based only on acute data. It is not that it is desirable, only necessary, given societal considerations. Thus, it is certainly easy to agree with the scientific desire of the authors of section 2 but it is unfortunately "too ivory-towerish." Regulatory agencies need to make the best judgement given limited data. While the thrust of this section, therefore, is solid scientifically, it misses the essential point that the regulatory agencies need assistance on making the best judgement when inadequate data exist.

Section 3 - Data for Analysis of Mutagenic and Carcinogenic Toxicity

The authors of Section 3 developed a three level ranking system of inadequate (I), minimal (M), and adequate (A) in their judgements of both mutagenicity and carcinogenicity data. It should be noted that there are a

number of toxicity ranking systems that are qualitative nature and require professional judgment. Some of these ranking systems are more elaborate than others. The present system is quite streamlined and reasonable. However, the system does not allow the specific incorporation of such important parameters such as predictive reliance of the model for humans, dosages used in the studies, etc. No specific way to differentially weight the respective assays were shown. Given the extensive data bases that exist for the list herbicides with respect to mutagenicity as well as carcinogenicity, integrated differential qualitative ranking scale systems would be useful, especially for the evaluation of mutagenic relevancy. Consequently, the qualitative ranking system proposed in the document is a reasonable initial scientific judgement system but it is not sufficiently helpful in the development of a method for quantitatively integrating the most relevant data in a manner that can be applied to making professional judgements on the mutagenic potential of these agents. Perhaps the best initial integrative approach I have read was reported by Dr. David Brusick in this 1980 book Principles of Genetic Toxicology.

A further note, at least in relative terms, is the generally robust nature of the available data bases in the present context. While there is always a need for a better understanding and therefore more research, the relative magnitude of the data allows those involved in assessing risk an initial handle. For example, amitrole and atrazine have 14 microbial assays for mutagenicity, dalapon - 8, 2,4-D - 10, Dicamba 6, fosamine 2, glyphosate 2, hexazinone, 1, picloram 8, simazine 4, tebuthiuron 1, triclopyr 3. These assays usually involved four testers strains with and without an S-9 fraction. In addition, the microbial assays were only one

part of a much broader battery of genotoxic assays which considered other types of genetic damage. The point is that often the risk assessor has available considerably more data with many of the herbicides presently evaluated to make a professional judgement about possible risk than in most other toxicologically based circumstances.

Section 4 - Data for Analysis of Reproductive and Developmental Toxicity

This section provided a generally clear and descriptive assessment of the procedures for the conduct of developmental and reproductive toxicological studies. The authors also presented a qualitative ranking system for developmental toxicity based on a four point system. In tables listing the actual reproductive and developmental studies the authors provided a qualitative ranking on the adequacy of the tests to satisfy the criteria of a reproductive and/or teratology study. These responses were usually given as one of the following: Invalid, incomplete, inadequate, unacceptable, unknown, minimum, acceptable, valid. It is assumed that these terms are the terms of the papers that the authors of the section cite and not their own. However, this range of terms is somewhat confusing, and apparently redundant. Definition of terms is essential but not included. Also, the four point qualitative ranking scale as developed by the authors was not used. There was an exceptionally strong reliance on the judgement from the California Department of Food and Agriculture Reports.

Section 5 - Data for Analysis of Immunotoxicity and Neurotoxicity

This section provided a very brief summary of the neurotoxicity and immunotoxicity of the herbicides to be used. The authors commented briefly on testing requirements to detect neurotoxicity. The only specific test is the delayed neurotoxicity evaluation which is required for all new organophosphates. However, it is likely that signs of significant neurotoxicity would be identified in the acute, subchronic and chronic toxicity testing. As for immunotoxicity no specific tests are required for pesticide registration. However, normal toxicological evaluation are likely to detect some alternations in immune function as reflected for example in thymus and spleen histology, and changes in the hematopoietic system.

Section 6 - Human Epidemiology

This section briefly described available epidemiological studies of a cohort and case-control nature. No attempt to provide an analytical assessment of the data were offered.

Impact of the University of Washington

Analysis on Risk Decisions

The report has accepted in general the approach used by the University of Washington investigators in classifying the adequacy of the toxicological data base with respect to specific endpoints of concern. However, it appeared that LAI tended to accept EPA characterizations of the data base as compared to those usually offered by the State of California as cited in the University of Washington report. If the California interpretations had been accepted more inadequate characterizations would have been noted. For example, Table IV-24 for reproductive and developmental effects would have had more herbicides characterized as not calculated due to inadequate data. Regardless of this characterization, NOELs were calculated for each agent even if the data were "inadequate information available for evaluating toxicity." This seems potentially contradictory. This phrase suggests that a NOEL could be derived from those data. I suggest that one consider using the phrase just below, that is, use the phrase "very tentative estimate." This reflects the uncertainties in data base clearly enough. Thus, by accepting the University of Washington approaches I do not see any change in final numbers calculated.

General Comments:

While I have carefully read Ames' article on the HERP rationale and can sympathize with his view, the HERP value is not very meaningful except to a narrow group of professional risk assessors. If this document is to be read by the general public HERP values will not be particularly helpful.

p. 5 - Appendix D

Field studies of workers have consistently shown that inhalation exposure represents only a small part of the total exposure, so doses to the general public in this analysis have been calculated only for dermal and dietary results.

Questions: need to see the relative contribution from inhalation
need to see the absolute contribution from inhalation

p. 13 - Appendix D

"Inhalation exposure was not estimated because none of the herbicides in question is a specific lung toxicant and because the worker field studies have consistently shown inhalation exposure to be an insignificant fraction of the total herbicide dose received (USDA, 1984).

1. Comment on the lung toxicant is not very relevant and should be omitted.

2. The real issue is dose; the document should show a summary of the real data; this would allow outside reviews to better evaluate your judgement.

see page 32 - Appendix D.

Appendix D, Section 5, pp. 1 and 3

There is mention made that the lowest NOEL is used when evaluating systemic and reproductive effects. Also, mention is made that the NAS Safe Drinking Water Committee and the toxicology text book by Klaassen et al. recommend the use of the highest NOEL. The writ up on pg. 1 does not distinguish which approach should be used while pg. 3 suggested that the lowest NOEL be used. This issue should be clarified and the course of action justified. It is not clear to me why the EPA and the NAS apparently differ on this point. It has been my understanding that the highest NOEL was always used.

Appendix D, Section 5, pg. 4

The systematic NOEL (mg/kg/day) listed on p. 4, Table 5-1 have 4 different values than those given in Table IV-19, pg. IV-101 of the other document. The differences are for asulam, atrazine, dicamba, and triclopyr. On appendix page H-14, evidence is given as for 3.7 mg/kg/day and 1 mg/kg/day for atrazine. Please clarify and check other possible discrepancies.

Also, it is unclear to me how the reproductive/teratology NOELs are derived for Table 5-1 (on pg 4) and how these related to values in Table IV-20 on pg. IV-102 of the other document.

p. 5 - The lowest margins of safety for realistic and worse case exposures related to amitrole. This prediction seems to be based on a single rat study that showed a thyroid type effect. It appears that a thyroid NOEL of some 50-fold higher was shown in Hazelton labs 1959 study. Since the thyroid effect (0.025 mg/kg/day) NOEL appears to drive risk appraisal, the study on which it is based should be analyzed carefully for its applicability to risk assessment procedures.

p. 11 - In the discussion of the public risk to brown and burn operations risks are calculated based upon estimated exposures. The assessment concluded that there is very little risk of adverse health affects. While the calculations were appropriate, it needs to be recognized that only very limited data exist on inhalation exposures to the herbicidal agents of concern. Since toxicity could vary significantly according to route of exposure, the possibility that unknown hazard may exist with an inhalation exposure should not be quickly dismissed. Even if the agents did not cause toxicity to lung tissue when administered by gavage or in food does not preclude the possibility that the lung or other tissues may be affected differentially by the different exposure routes.

p. 18 - I am concerned primarily about the estimation of fatality associated with accidents. The Table 5-8 on pg. 19 provides the margins of safety for LD₅₀ values for both concentrate and spray mix. These values

need to be put in some better form to enhance understanding. Thus, what does it mean in terms of probability of fatality with a MOS of less than 10 for the concentrate LD₅₀ for triclopyr, dicamba, 2,4-DP, 2,4-D, and atrazine. One paper that might be helpful is that of Dourson and Stara (Reg. Tox. Pharm. 3: 224, 1983) who reviewed the responses of rodents to acute toxicity for a large number of agents. They found that a 10-fold decrease in dose would reduce the median exposure for an LD₅₀ to below the general range expected to result in death from 92% of the individual compounds studied.

General Comments:

I believe that it would be beneficial if the document identified the numerous assumptions that a cancer risk assessment employs and then identify how the document has dealt with the assumptions. It would be instructive and place compactly into a better perspective to what extent the worse case analysis approaches the challenge of risk estimations. A particular concern is whether there is such a cascading of multiplicative risk factors that the estimation of risk is even beyond what could be legitimately called a worse case analysis.

Received from Dr. Ed Calabrese

2/11/88

Human Risk Associated With
The Spray Program For Vegetation Control

The Draft Environmental Impact Document represents an attempt to assess risk for projected environmental exposure of herbicide application to workers and the general public.

My judgment concerning the proposed risks and how they were dealt with is as follows:

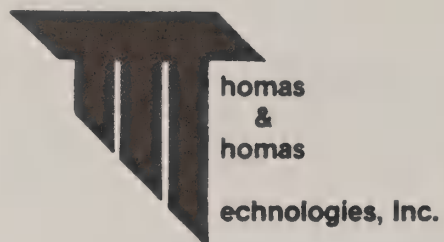
The document has used very conservative methodologies to estimate risk. For example, the document utilizes a one-hit regression model and the upper 95th confidence interval for estimating cancer risk. The studies upon which these conservative regression approaches were based are very widely believed to be with highly sensitive animal models using study design which provide a maximum possible exposure. Through the hazard assessment and dose-response component of the document there is a clean and unmistakable effort to ensure that risk is not underestimated but in fact to err on the side of safety. The consistent erring on the side of safety is likely to result in a cascading process of excess risk accumulation that any ultimate projected risk is likely to be of a magnitude greater than what is realistic.

Secondly, the approaches to quantify exposure, especially to the general public, involve numerous assumptions concerning drift, biodegradation, depth of drinking water contaminants and many

more. In these instances, the document provides a consistent effort to again err on the side of safety with its judgments. The judgments will lead to extreme overestimates of exposure.

The bottom line is that it is difficult to conceive of any realistic circumstances occurring in which a significant projected risk would occur to the general public. In addition, there is extreme planning and careful attention to details on the conduct of spraying activities. This would add a very significant influence.

COMMENTS OF DR. RICHARD THOMAS



February 3, 1988

Andrea Myslicki
LABAT-ANDERSON Incorporated
1111 19th Street North
Arlington, VA 22209

Dear Andrea:

I have completed a preliminary analysis of the University of Washington section of the risk analysis done for Region 6. I will begin with a general summary followed by specific comments. The specific comments highlight differences between the analysis done by Labat-Anderson and the U. of Washington. In most cases, there was not sufficient time to complete the analysis required to recommend resolutions to those differences.

GENERAL SUMMARY

The U. of Washington uses a cancer relative risk analysis which is not "generally accepted practice". It is a method proposed by Bruce Ames that is not generally recognized by the toxicology and regulatory community. The U. of Washington analysis shows the risk of using herbicides to be, except under the most extreme exposure assumptions, much less risky than eating one peanut butter sandwich. However, this is not reported. The U of Washington presents an inflated impression of the concern for 2,4-D as a human carcinogen and presents what appears to be a biased discussion of the literature. The epidemiology section in Appendix H includes a significant amount of literature not relevant to the program of the Forest Service.

There are major differences in information base and conclusions on reproductive/developmental and/or systemic toxicity for Amitrole, Atrazine, Bromacil, 2,4-D, Dalapon, Dicamba, Diuron, Fosamine, Hexazinone, and Tebuthirion. Despite these differences, if one strictly uses the Margins of Safety (MOS's) as a reference point, there are no differences in results of analysis for the public scenarios for Bromacil, 2,4-D, 2,4-DP, glyphosate, hexazinone, simazine, and picloram. In addition, the differences in MOS that are calculated do not appear to be biologically significant. However, I would want to do further analysis to confirm this impression. Dalapon, Diuron, Dicamba, and Fosamine are presented by the U. of Washington as having significant missing information. This is the biggest difference in the presentations by U. of Washington and Labat-Anderson.

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Finally, I noted that in Chapter 4, fear of herbicides is presented as an environmental impact while almost no non-psychological health effects are mentioned. I think this is an unsatisfactory approach to a human health effects section of an EIS.

A COMPARISON OF THE LAI AND U. OF WASHINGTON ANALYSES AND
A CRITIQUE OF THE EIS.

ANIMAL CARCINOGENICITY

On page IV-94, the U. of Washington makes its case for using the Ames (1987) method of calculating cancer risk. The rationale, they cite for using this method over conventional risk assessment is "insufficient scientific basis for such modeling". They then state that the HERPS are useful for comparing relative risk among the herbicides.

What the U. of Washington does not present is the fundamental conclusion that Ames et al (1987) has reached. This is represented by the following quotation from his 1987 paper:

"the analysis on the levels of synthetic pollutants in drinking water and of synthetic pesticide residues in foods suggest that this pollution is likely to be a minimal carcinogenic hazard relative to the background of natural carcinogens. The result is consistent with the epidemiologic evidence. Obviously, prudence is desirable with regard to pollution, but we do need to work out some balance between chemophobia with its high cost to the national wealth, and sensible management of industrial chemicals."

It is this conclusion, a direct result of the HERP method of analysis, that is quite controversial at this time.

The U. of Washington should have noted that, according to Ames et al. (1987), the HERP for a peanut butter sandwich is 0.03 and the FDA allows a HERP of 0.3 for aflatoxin. This HERP is 30 times more relative risk than atrazine "worst case for a nearby resident" and 10000 times more than the picloram and asulam LOEL HERPS.

HUMAN EPIDEMIOLOGY

The discussion on epidemiology and cancer on pages IV-95 to IV-100 gives an inflated impression of the scientific community's concern for the potential carcinogenicity of 2,4-D and 2,4-DP. There is no current concern for lung cancer, stomach cancer, or leukemia. In the case of lung cancer, Lynge (1985) states that

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no conclusion is possible. The data for the later two types arises from individuals exposed to 2,4,5-T containing TCDD. There has been concern about Hodgkin's disease, non-Hodgkins lymphoma, and soft tissue sarcoma which has recently focused on non-Hodgkins lymphoma. In addition, the discussion fails to cite any articles critical of the evidence, such as Colton (1986), Coggon et al., (1982), and an unsigned editorial in Lancet (1982).

Appendix H - Human Epidemiology- Most of the studies reviewed are on exposure to 2,4,5-T. Studies on reproductive outcomes do not mention 2,4-D. One of the two studies on neurological disorders mentions 2,4-D but also is confounded by the presence of 2,4,5-T. In the references cited, there are no listings for the Singer and Moses (1982) and Suskind and Herzberg (1984) studies.

COMPARISON OF DATA IN REPRODUCTION AND DEVELOPMENTAL EFFECTS SECTIONS OF APPENDIX D WRITTEN BY LAI (PAGES 7 - 13) WITH DATA GIVEN IN TABLES COMPILED BY THE U. OF WASHINGTON IN APPENDIX H (PAGES H- 93 TO H-113)

Amitrole - According to LAI, EPA says reproductive data is adequate, developmental is not. This is exactly the opposite conclusion of U. of Washington, Table IV-20. U. of Washington reports a developmental toxicity study with NOEL of 4 mg/kg which LAI does not mention. U. of Washington takes NOEL of 5 from a reproductive study and divides it by 10. Based on data on developmental studies presented by the U. of Washington, the developmental NOEL should be 1 mg/kg.

Asulam - U. of Washington Table and LAI agree.

Atrazine - Cannot determine how LAI got 100 mg/kg for a NOEL. U. of Washington used a rat study where NOEL > 100ppm was reported. (pg H-93). No concordance between studies described on page H101-102 and LAI discussion on page 8,9.

Bromacil - LAI does not mention a 3 generation study reported by U. of Washington. U. of Washington does not mention rabbit study. LAI does not mention that the rat study exposure was by inhalation.

2,4-D - U. of Washington calculates 1.25 for developmental NOEL based on an LEL in a rat teratology study. I think they looked at skeletal abnormalities as separate from fetotoxicity. U. Of Washington is saying 2,4-D is a developmental toxicant. I do not think the evidence supports this conclusion.

2,4-D,P - Reproductive studies agree. There is still an issue of the Developmental NOEL being less than maternal toxic NOEL which neither group mentions. U. of Washington suggests that it would be a developmental toxicant (Table IV-20) and then says that data is Adequate.

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Dalapon - A three generation rat reproductive study listed by U. of Washington but not reviewed by LAI. Two developmental studies are not mentioned by LAI but are listed by U. of Washington. U. of Washington appears to have more accurate portrayal of facts in Table IV-20 than LAI does on page 10.

Dicamba - There are three studies on reproductive effects listed by U. of Washington but not mentioned by LAI. As to NOEL >500, these data are called marginal by U. of Washington. For developmental toxicity data the issue surrounds the use of the rabbit studies. EPA calls the developmental NOEL 2.5. U. of Washington uses the study on rabbits with a NOEL of 0.5. This is a range finding study. They do not mention the nature of this study and they call the data adequate which does not appear to be consistent with their definition of this term as I understand it. They also imply in Table IV-20 that Dicamba is a developmental toxicant.

Diuron - LAI reports only 1 reproductive study. U. of Washington reports 2 reproductive and 2 developmental studies. U. of Washington calculates a developmental NOEL by dividing the LEL in one developmental study by 10.

Fosamine - U. of Washington lists a one generation reproductive study NOEL of 250 mg/kg not mentioned by LAI. U. of Washington does not report either study reported by LAI for developmental toxicity, but does report a study using Krenite that had a NOEL of 100ppm. Fosamine content of Krenite is calculated by U. of Washington. Thus LAI uses NOEL of 500 for reproductive/development and U. of Washington uses 250 for reproduction and 21 for development.

Glyphosate - appears to be in agreement.

Hexazinone - LAI uses a NOEL of 125. The lowest NOEL is clearly 50 although LAI mentions a rabbit developmental study with a NOEL of 20 mg/kg. LAI in Region 5 uses 50 for Hexazinone as well, so this appears to be a mistake that will need to be corrected.

Picloram - There is no concordance between the discussion on page 12 by LAI and data on teratology presented on page H-110 of U. of Washington.

Simazine - U. of Washington says the NOEL for 3 generation rat was greater than 100. A developmental study in rabbits was not mentioned.

Tebuthirion - Three generation rat study not mentioned by LAI but listed by U. of Washington. NOEL in study mentioned by LAI is said to be > 400 ppm, not equal to 400 ppm. Agreement on developmental studies descriptions. U. of Washington uses > 20 for reproductive and between 25 and 237 for developmental NOEL.

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Triclopyr - Agreement on the reproductive study. Difference between understanding of value of developmental studies between U of Wash. and LAI.

COMPARISON OF SYSTEMIC NOELS

Asulam - U. of Washington says NOEL is 20 mg/kg in Table IV - 19. LAI uses 50 mg.kg. I cannot find where they get 20. It may be a misprint.

Atrazine - U. of Washington uses NOEL of 1 in contrast to LAI's 3.7. It appears U. of Washington used a teratogenicity study for establishing the systemic NOEL.

Triclopyr - U. of Washington uses Quest et al, 1977 study where NOEL is listed as 0.5. It appears that LAI does not discuss that study.

OVERALL DIFFERENCES IN NOELS CHOSEN BY LAI AND U OF WASH.

See attachment A.

DIFFERENCES IN ANALYSIS OF MOS BETWEEN LAI AND U OF WASH.

Table IV-24 through IV-26 present the MOS's calculated by U of Washington. They use letters instead of numbers. If you translate the letters to their equivalent numbers and compare with LAI data in Attachment C of Appendix D for the scenarios the U. of Washington picks, then for:

Table IV-24, Public safety under routine scenario, the only difference is for Dicamba reproductive. Dalapon, Diuron, Dicamba, and Fosamine are presented as having "missing information". However, no analysis or discussion of the meaning or importance of this designation is made.

The reasons for the difference is:

Dicamba - LAI uses a NOEL of 2.5 and U. of Washington uses a NOEL of 0.5.

In addition I note about Dalapon that LAI statement on page 10, section 3 does not agree with USDA (1984) and that for Diuron, there does not appear to be alot of information. Not reviewed by USDA (1984) - Because of the lack of information a MOS of 1000 might be a more conservative reference point for safety. We should discuss this.

Table IV - 25, backpack sprayers - there are differences between atrazine reproductive, tebuthiron reproductive, and glyphosate systemic MOS's. Dalapon, Diuron, Dicamba, and Fosamine are presented as having missing information, but no analysis or discussion of the meaning or importance of this designation is made.

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The reasons for the differences are:

Atrazine - LAI uses reproductive NOEL of 100. U. of Washington uses 5. USDA (1984) has no data that sheds light on this.

Glyphosate systemic due to the fact that U of Washington uses 20 mg/kg for bottom of range so MOS is 80.

Tebuthiron - Reproductive MOS incorrectly calculated by U. of Washington. They use >20 for NOEL and but then report MOS is less than 100.

Table IV - 26, public worst case - there are differences between Asulam systemic, Tebuthiron reproductive, and Triclopyr systemic. Dalapon, Diuron, Dicamba, and Fosamine are presented as having missing information no analysis or discussion of the meaning or importance of this designation is made.

The reasons for the differences are:

Asulam - Because U of Wash uses 20 for systemic and not 50, their MOS is 49.

Tebuthiron - Same problem as in table IV-25. MOS should be >41 to 486.

Triclopyr systemic is lower because U of Wash uses 0.5 instead of 2.5.

FURTHER COMMENTS ON U OF WASHINGTON REPORT AND EIS DRAFT

Table IV-17 Why is amitrole public aerial higher than backpack applicator? Is backpack low?

Page IV-93, para 1 - I do not understand the discussion of NOEL's and LOEL's and 10%?

Page IV-93, para 5,6,7 - This is an oversimplification. Our confidence or lack of it depends on the type of study used, our knowledge of the mode of action, metabolism, and the safety factor employed.

Page IV-102, Table IV-20 - The analysis of adequacy of data appears to be biased. For example, developmental NOEL for Dicamba is listed as being adequate because the value is unlikely to change. However, there are studies with rats which show a much higher NOEL. Therefore this information should be of uncertain character. It appears that the U. of Washington is unwilling to acknowledge that some studies may over-estimate the toxicity in regard to human health. Also Asulam, atrazine, diuron, glyphosate, hexazinone are said to show "evidence for adverse developmental effects, primarily minor abnormalities observed". 2,4-D, 2,4-DP, dicamba, and fosamine show evidence for

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adverse developmental effects, including some major malformations. This makes it appear that these chemicals are developmental toxicants.

Page IV-111 - Uses LOEL HERP's for worst-case applicator to discuss cancer hazard.

Page IV-114 - Glyphosate - Statement on cancer potency contradicts statement on page IV-110, para 5.

Page IV-115, para 3 - What is a "moderate potential" for carcinogenicity?

Page IV-115,116 Summary - Chemicals listed as of Particular Concern are a reasonable list. Again Diuron and Fosamine are listed as having missing information but no analysis or discussion of the meaning or importance of this designation is made.

Page IV-117 - Discussion of inerts suggests that there are toxic compounds in the herbicides in this program that may be untested.

Page IV-117, para 7 - conclusion is quite different from LAI approach.

Page IV-118, para 1, Use of the term dioxin instead of dioxins, a code word for TCDD.

Page IV-118, para 3 - Brings up the fear issue. Claim is that fear is a legitimate environmental impact. Note that no actual health effects are mentioned.

Page IV-119, para 3 - Elevates the importance of perceived risks.

Page IV-119, para 5,6,7 - States that risk to the general public will be small and less than to workers.

Page IV-120, para 3 - Risk from Inerts - Incomplete evaluation.

Page IV-121, para 2 - I do not understand what the point is here?

Page IV-123, para 1,2,3 - Most of material presented is not relevant to the discussion.

Page IV-126 - Alternative A - reduced potential chronic toxicity and perceived risk; Alternative B - Only problem is with public perception.

Note - Conclusions by U. of Washington do not point out potential effects they believe might occur. Example is page IV-119,120 where the best they can say is there is concern. Remarkable lack of information on human effects in the analysis.

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OTHER POINTS

Page H-6 Argument about mixtures is directly contradictory to that made by LAI.

U. of Washington relies heavily on California tox one-liners.

Page H-2, last three paragraphs - U. of Washington fails to distinguish false positive and negatives that are statistical in nature and those that are biological, i.e. model test system is not good model for human toxicity.

Page H-7 - Discussion of inerts is exactly opposite of the point of view presented by LAI.

Page H-9 - Discussion of uncertainty in data does not reflect state of the art in toxicology. For example, a wide variation in results between species may not mean very much in terms of predicting human toxicity. Several animal species are used primarily to search for the best model for human toxicity. Uncertainty arises when there is doubt about which animal model is most appropriate. Once there is consensus on which animal model is appropriate, results in other species will often be considered unimportant. The concern here is that the simplistic discussion of the methods of toxicology presented here will confuse the general reader.

Page H-10 is discussion of U. of Washington philosophy on uncertainty of total results.

Summary- 10, Figure 5-3 - Risk Index - Method of calculation undefined.

Summary - 19, para 3 - Public risk is not defined anywhere in the document.

Appendix C - Neither LAI or U. of Washington takes into account Table C-2 in the analysis of public risks. 2,4-D and glyphosate account for 69% of the herbicides used. Adding picloram and triclopyr, that accounts for 84%. I believe LAI does take this information into account for analysis of risk to workers.

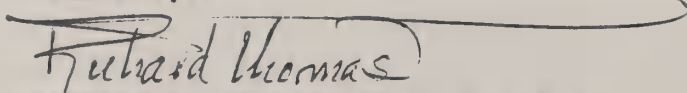
There is no information provided on the relative toxicity of different formulations.

In conclusion, I do not feel I could defend the EIS as a whole as it is currently written. I think this document needs considerable further work and rewriting in order to produce a defensible document. The methods used by the U. of Washington are not "generally accepted" and the results are either inconsistently presented or lacking when it was decided there was

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Inadequate data. When the U. of Washington analysis is combined in the same document with the Labat-Anderson analysis, the document is flawed in logic and internally inconsistent in basic scientific analysis.

Sincerely,

A handwritten signature in cursive script, reading "Richard D. Thomas". The signature is written in dark ink and is positioned above the printed name and title. A horizontal line is drawn across the page, starting from the left margin and extending past the signature.

Richard D. Thomas, Ph.D, D.A.B.T.
Consulting Toxicologist

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ATTACHMENT A

OVERALL DIFFERENCES IN NOELS CHOSEN BY LAI AND U OF WASH.

	U OF WASH SYSTEMIC	LAI	U OF WASH REPRO	DEVELOP	LAI REPRO/DEVELOP
AMITROLE	0.025	0.03	0.5	4	5
ASULAM	20	50			
ATRAZINE	1	3.7	>5	5	100
2,4-D			5	1.25	5
DALAPON			(>300)	500	300
DICAMABA	0.125 - 3.0	25	>500	0.5	300
DIURON			(>125)	12.5	6.25
FOSAMINE			(>250)	(21)	500
HEXAZINONE		50	50	125	
GLYPHOSATE	20 >500	31			
TEBUTHIRON			>20	>25<237	90
TRICLOPYR	0.5	2.5			

COMMENTS ON THE ENVIRONMENTAL IMPACT STATEMENT:
MANAGING, COMPETING AND UNWANTED VEGETATION IN THE NATIONAL
FORESTS OF THE PACIFIC NORTHWEST REGION

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We were requested to review and comment on Chapters II, IV and Appendix H of the Draft Environmental Impact Statement (EIS) and specifically to address our comments to four questions:

1. Accepting as given the evaluation and interpretation of toxicologic and epidemiologic studies represented in the Appendices, are human health risks well characterized in Chapter II and IV of the draft EIS?
2. Given how human health risks are described in Chapters II and IV, does the draft EIS draw clear and appropriate conclusions from this characterization of risks?
3. Given the characterization of health risks and the conclusions drawn from this characterization of risks in Chapter II and IV, is the analysis of health risks adequately and appropriately incorporated into the evaluation of alternatives?
4. What recommendations would you suggest for management of risks related to possible exposures to herbicides in

forests, considering both forest workers and the general public?

Specifically we address our comments, as requested, to "the approach which was used to arrive at conclusions on human health risks for use of herbicides in vegetation management." (Letter from Gary L. Larsen, February 11, 1988).

However, there is an added restriction that poses a problem.

The instructions contained in the covering letter request the reviewer to evaluate the characterizations given in Chapters II and IV of the health risks from exposure to herbicides "accepting as given the evaluation and interpretation of toxicological and epidemiological studies presented in the Appendices". Consequently, the reviewer is asked to accept the basis on which the EIS builds its recommendations and conclusions concerning effects on human health.

This limitation is sometimes justified to prevent a fruitless argument over the interpretation of the results of relevant studies. However, in the present instance, this limitation is an unjustifiable restraint on the evaluation of the EIS characterization of the health effects of phenoxy herbicides (and possibly also of other chemical agents). This is because a substantial number of relevant studies are not included in Appendix H and the relevant results of other

studies are not mentioned. Consequently, it is very difficult to evaluate the conclusions and characterization of the risks in the report because these characterizations are apparently based on only part of the relevant data. (Also given the missing data, the EIS does not appear to satisfy the required "Worst Case Analysis" [EIS, Chapter 1, page 5]).

Therefore, the EIS review of the epidemiological studies relevant to phenoxy herbicides is discussed as part of our comments to QUESTION 1 and before addressing the suitability of the proposed "Alternatives".

QUESTION 1: (Accepting as given the evaluation and interpretation of toxicologic and epidemiologic studies presented in the Appendices, are human health risks well characterized in Chapter II and IV of the draft EIS?)

This question can not be answered without evaluating the data used by the U.S. Forest Service Report to evaluate health effects.

Reproductive Effects

The summary on page H-135 of Appendix H of epidemiological studies of reproductive effects from exposure to phenoxy herbicides lists the results of only four studies, excluding the reference to Hatch, which is a review. There appears to be no logical criteria for the

inclusion or exclusion of studies in this list. For example, the study by Townsend et al (1982) of conceptions among the wives of men who worked in a building where chlorophenols were made is included in this list even though it is not as relevant as several studies of exposure to phenoxy herbicides which were not included (see below).

The list of four epidemiological studies in Appendix H includes two ecological studies, by Nelson et al (1979) and Thomas (1980), which did not find an association between phenoxy herbicide use and unfavorable reproductive outcomes. Remarkably, the list *does not mention five other ecological studies, all* of which find a positive association between phenoxy herbicide use and unfavorable reproductive outcomes (Balarajan, 1983; EPA, 1979; Field & Kerr, 1979; Hanify et al, 1981; Meselson et al, 1971). In addition, the list does not include the Ranch Hand study of American Vietnam war veterans who sprayed herbicides in Vietnam (Lathrop et al, 1984). This study found a statistically significant increase in all birth defects combined after adjusting for the effect of five other risk factors. There is no mention of the extensive study by Erickson et al (1984) of another group of Vietnam veterans exposed to phenoxy herbicides in Vietnam or of a series of Vietnamese studies which have been reviewed in two western journals (Constable and Hatch, 1985; Sterling and Arundel, 1986).

Neurological and Other Effects

Appendix H summarizes the results of only two epidemiological studies of neurological effects and does not mention other effects such as fatigue and liver toxicity. Other studies, both of phenoxy herbicides and related compounds such as chlorophenols, are available. For example, Sterling et al (1982) found significant increases in dermatological and general respiratory symptoms, and impairment of general neurological functions among sawmill workers exposed to chlorophenols in spraying and dripping operations. Additional relevant studies on neurological effects, fatigue and liver toxicity have been reviewed by Moses et al (1984).

Cancer

The discussion of epidemiological studies of cancer in chapter IV is incomplete and requires clarification in several places. One problem is that exposure to 2,4,5-T, which contains 2,3,7,8-TCDD, was possible in many of the studies of phenoxy herbicides. This problem is noted in the text, but the text fails to emphasize studies where exposure was probably limited to 2,4-D or other phenoxy herbicides which are not known to contain 2,3,7,8-TCDD.

Two studies (Zack & Suskind, 1980; Theiss et al, 1982) have been included in this section even though there was no exposure to phenoxy herbicides at all. The exposure in these two studies was limited to dioxins and other compounds as a

result of industrial accidents. The relevance of these two studies to the effects of exposure to 2,4-D or 2,4-DP should be qualified.

All studies which addressed the health effects of exposure to phenoxy herbicides other than 2,4,5-T should be discussed separately. The Swedish study by Erikson et al (1981) found an increase in risk in Soft Tissue Sarcoma (STS) for exposure to phenoxy herbicides other than 2,4,5-T. This fact is not mentioned in Chapter IV or in Appendix H (p 133), even though this is a very important finding. The study by Newell et al (1984) of New Zealand sheep is only mentioned in the appendix even though it is a very important study in respect to 2,4-D. Both Appendix H (p. 132) and the Chapter IV summary of the EIS evaluation of the carcinogenicity of phenoxy herbicides *completely fail* to mention that the results of the study by Hoar et al (1986) largely concern exposure to 2,4-D only. Table 3 of the study by Hoar et al (1986) presents the results of an analysis of the risk of non Hodgkin's Lymphoma (NHL) from exposure to 2,4-D which provides strong evidence for a dose-response effect.

Conclusion

The EIS omits several important and relevant studies on exposure to phenoxy herbicides and health effects. The results of these studies, particularly those of cancer and exposure to phenoxy herbicides other than 2,4,5-T, suggest

greater concern for the potential for health effects or cancer as a result of exposure to 2,4-D or 2,4-DP than implied in the Chapter IV summary on page 112.

There are also two relevant epidemiological studies that were published in 1987 but which were not included in either Appendix H or the Chapter IV summary. Vineis et al (1987) found a statistically significant increased risk of 15.5 for STS for women probably exposed to phenoxy herbicides between 1950-1955 in Italy. Woods et al (1987) evaluated the risk of STS and NHL from exposure to chlorophenols or phenoxy herbicides in Western Washington. The study did not find an increase in STS from phenoxy herbicide exposure, except for individuals from a Scandinavian background, but it did find a statistically significant increased risk for NHL of 4.8 for forestry herbicide applicators and a risk of 1.71 for all individuals potentially exposed to phenoxy herbicides for 15 years or more.

QUESTION 2: (Given how human health risks are described in Chapters II and IV, does the draft EIS draw clear and appropriate conclusions from this characterization of risks?)

Page 94 of Chapter IV and Table IV-27 present the results of a new and so far not widely used method (developed by Ames) of characterizing the risk of cancer from exposure to herbicides. Conversely, pages 21-28 of

Appendix D gives the widely used method which calculates lifetime cancer risks from applying a one-hit risk model to the results of animal studies. Though there may be merits to the method used by Ames, Chapter IV should include a table which gives the results of the conventional method for presenting cancer risks. It is much easier to determine the significance of a cancer risk from a table based on the lifetime risk of cancer mortality per 100,000 exposed individuals than to interpret the extremely small numbers obtained from the LOEL HERP. The failure to include conventional methods in the main text leaves the reviewer with extremely low numbers which are difficult to interpret.

An additional problem with the estimation of risks is that they are based only on the results of animal laboratory studies. No mention is made in Appendix D of the cancer risk from 2,4-D exposure observed in several epidemiological studies. The appendix should have also estimated the lifelong risk from the results of these epidemiological studies, particularly the study by Hoar et al (1986) of NHL. The relevant epidemiological studies probably suggest that the lifetime cancer risk from 2,4-D or 2,4-DP exposure are substantially higher than the risks estimated from the results of animal studies.

In conclusion then, human health risks as derived by the EIS are difficult to interpret and for that reason the

draft EIS does not draw clear and appropriate conclusions. No basis is provided for a Worst Case Analysis.

QUESTION 3: (Given the characterization of health risks and the conclusions drawn from this characterization of risks in Chapter II and IV, is the analysis of health risks adequately and appropriately incorporated into the evaluation of alternatives?)

Even though the characterization of risks are not adequate and appropriate, the seven alternatives (A to G), for managing unwanted vegetation appear to present all of the possible major choices for forest management. The fact that the EIS very likely underestimates the risk to human health does not change the problem of making a choice between the listed alternatives A through G.

While there is a wide range of alternatives, the difference between some of them is not quite clear. It would seem that the differences in practice between Alternatives B, D, and G are undefined. For example, the difference between preventive (Alternative D) and aggressive (Alternative G) use of herbicides and other methods of control is not clear. Alternative B (which describes present practices and is used as the "reference") is too aggressive now for some interested parties and less aggressive than it ought to be for other interested parties. Thus, whether Alternative B will be constrained toward a more aggressive application as in alternative G or a less

aggressive application as in alternative D will basically depend on who makes the decision about what is aggressive or preventive. For that reason these three alternatives are really a single alternative and will be so referred to hereafter as (BDG).

Alternative E presents a clear choice for using herbicide and differs from (BDG) because it prohibits aerial spraying. (In this respect the EIS does not sufficiently deal with the health implication to applicators in manual operations nor does it deal adequately with problems of drift during aerial applications.)

Alternative F is somewhat unclear. Obviously under that alternative not all fires will be proscribed. Not affected are (and must be) fire breaks, logging slashes, and natural fires. Alternative F mentions that burning of chemically treated vegetation is prohibited. That prohibition could have been included in (BDG) and E, as well. (In this respect, potential hazards of fire in areas that have been recently sprayed are inadequately treated in the EIS.)

Alternative A is desired by many affected individuals and, at the same time, historically has been strongly opposed by forest and agricultural interests including the U.S. Forest Service. While the use of some types of herbicides are proscribed in some countries (for instance Sweden) others may be substituted. Alternative A represents

an idealistic situation and it is unlikely to be implemented under present conditions.

Similarly, it is unlikely that Alternative C will be implemented.

In practice then, the choice appears to be between Alternatives E and (BDG) or between different restrictions and the method of application of herbicides.

Thus, despite the inadequacy with which human health risks are discussed in the EIS, a range of alternatives are presented and sufficient data exist today about the risks to human health on the one hand and necessary conditions of forest management on the other so as to come to some conclusion of what the choices are. These major choices appear to be:

1. Whether or not aerial application should be permitted;
2. Whether or not use of fire in some areas should be restricted;
3. Whether or not all herbicides should be treated equally (while data on the known toxicology of various herbicides of choice are described, the report does not directly draw conclusions on which herbicides are herbicides of choice in different situations).

QUESTION 4: (What recommendations would you suggest for management of risks related to possible exposures

to herbicides in forests, considering both forest workers and the general public?)

There are risks to human health due to the use of any method of vegetation control and there are benefits to various economic, political and social interests from appropriate methods of forest management. It would appear that the EIS, combined with other information, could be used to support the following recommendations; especially to risks related to the use of various herbicides.

1. Herbicides, and in general, all chemicals should be used as little as possible in forestry. The rule should be that management procedures which do not use chemicals should be used if their costs are within a realistic budget level.
2. If chemicals are used they should be applied directly. Aerial applications should be terminated in general because they constitute the highest risk to the public. In manual application care needs to be taken that herbicides are not applied near water resources and that applicators are properly trained and protected.
3. The use of fire for control should not closely follow application of chemicals. If possible, fire management ought not to be used in chemically treated areas of the forest within the same year.

4. As much as this is possible there should be discrimination between the types of chemicals used for different control methods.
5. Replanting of harvested areas should be done as close in time as possible to finishing of cutting so as to decrease the problem of unwanted vegetation obstructing the growth of seedlings.
6. The Forest Service should institute the practice of monitoring compliance (of the Forest Service as well as of industrial forest activities).
7. Monitoring should be combined with systematic research of cost and effectiveness of different methods of control. The fact needs to be faced that present estimates of cost and effectiveness are not so much based on actual research data but more on the collective opinions of individuals who are working in forest management. While these opinions certainly should be respected, they need to be verified by actual data.
8. Insofar as any management procedure must stay within a prescribed budget (the EIS stresses the need to stay within budget) budgets need to be formulated on a realistic basis, permitting choices that include manual applications of chemicals or manual removal of unwanted vegetation. Budgets that do not provide such choices

actually force a choice - that of heavy use of chemicals.

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March 25, 1988

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Dear Dr. Larsen:

I write to offer my comments on the DEIS on, "Managing Competing and Unwanted Vegetation." I apologize for not responding to your request in the initial timeframe. The document you sent me was much more extensive and detailed than I had imagined and I spent considerably more time on the review than I had planned. I hope that my comments are still of use to you.

Overall, I am very impressed with the thoroughness and thoughtfulness of the DEIS. It is an excellent document that, in considering short and long term risks, goes far beyond most other impact statements or risk assessments I have reviewed. This is not to say that I do not have criticisms and suggestions but rather to say that in spite of the pages of comments that follow I congratulate you on the quality of your analysis. You have incorporated some truly innovative and useful methodology in your work, such as the qualitative risk assessment, and have provided the reader with a fuller and more comprehensive evaluation of the situation than is typical. I hope that my criticisms are taken in the constructive spirit in which they are offered. Overall, I strongly support the formulation of your document.

My main criticisms, as outlined on the attached document, are two-fold. First, a variety of unusual hazards were identified and discussed in the document but were not incorporated into the risk assessment formulation. These considerations are of paramount importance, as you noted, but because of the rigid structure of the quantitative risk assessment model they cannot be included easily. For example, in Appendix H there is much discussion of statistical issues ranging from false positives and false negatives to statistical significance and power. Yet, these concepts and evaluations are not explicitly included in any of the analyses. There is mention of the potential hazard of "inert" components of pesticide formulations. Yet, no evaluation (can be or) is provided. Much data is regarded as inadequate by your toxicologists. These omissions and limitations bring into question the validity of any evaluation of the safety or hazard of any material or practice. Second, the exposure assessment is a weak link in your quantitative risk assessment. Typical practices and hazards are considered, but these represent only a small range of the likely or even possible exposures to be experienced over the years. Underestimation of exposure leads to underestimation of health effects. A more comprehensive review of this aspect of your study is warranted.

Overall, the document is a vast improvement over other risk assessment documents I have reviewed. While there are still some shortcomings to this document, I believe it represents a major step forward for those involved to be presenting the issues so clearly and forcefully with the intent of informing and educating. With additional improvements, I believe this effort for evaluating hazard and communicating with the public will be a model program for others to try to emulate.

In response to the four specific questions you posed, I summarize my views below and detail them in the attached comments.

(1) Accepting as given the evaluation and interpretation of toxicologic and epidemiologic studies presented in the appendices, the human health risks are moderately well characterized in Chapter II and IV of the DEIS. My chief concerns are the omission of risks due to inert ingredients, the inadequacy of the exposure assessment to model unusual occurrences, the inadequacy of toxicological information as detailed in the qualitative risk assessment and all the typical limitations of quantitative risk estimation (e.g., adequacy of statistical model, limited size of data sets). To the degree that the qualitative and quantitative assessments are used together, this allays some of my concerns. Finally, in view of the missing and inadequate data, one must constantly be aware of the tenuousness of any conclusions based on the database.

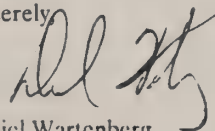
(2) Given how human health risks are described in Chapters II and IV, the DEIS has appropriate conclusions to the extent that they are provided, with the caveats noted in (1). Conclusions regarding choice of a particular alternative based on these risks is not provided, to my knowledge. So, the summarization (rather than conclusions) are appropriate given the risk assessments.

(3) Given the characterization of health risks and the conclusions drawn from this characterization of risks in Chapter II and IV, the analysis of health risks is adequately and appropriately incorporated into the summarization and comparison of the alternatives. However, given the limitations noted in (1), there are limitations at this level of analysis as well. Any conclusions based on inadequate testing are tenuous.

(4) My recommendation for management of risks related to possible exposures to herbicides in forests, considering both forest workers and the general public is Alternative D, with E as my second choice. The risk assessment suggests considerable hazard can result for widespread herbicide use. Nonetheless, economically efficient management of forest resources is an important consideration. Alternatives D and E offer what I believe is the best compromise between these competing concerns and is most in concert with the functioning of the forest ecosystem. What is important with the implementation of either of these strategies is careful consideration of the specifics of the management strategy and minimization of risk in the specific situation. Herbicides, when necessary, must be selected with ample consideration of both human and environmental risks and applications must be designed to exposure. In view of the problematic toxicological data, use of herbicides must be minimized whenever possible. We really cannot be certain how dangerous exposure to these chemicals is.

I thank you for inviting my comments and, at your request, would welcome the opportunity to further amplify any of the statements I have made.

Sincerely,



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Assistant Professor
Department of Environmental and Community Medicine

Daniel Wartenburg
March 25, 1988

Comments on, "Managing Competing and Unwanted Vegetation"

The draft EIS provided to me by the USDA Forest Service on, "Managing Competing and Unwanted Vegetation," is a comprehensive, impressive and well written document. The authors are to be commended for the thorough and thoughtful work, and the broad range of alternative strategies that are considered for controlling vegetation. As requested, I will focus my comments on Chapters II and IV and Appendix H, although I have read the entire document to enable me to put those sections into perspective.

Fundamental to the goal of this analysis is the observation presented in Chapter 1, that unwanted vegetation is part of an ecological system. The vegetation plays an intricate role in the functioning of a complex ecosystem and the key to effective management is an understanding and utilization of this system. By manipulation and direction of the ecosystem's own resources one can manage and control the system in a effective and efficient manner with minimum degradation and risk to human health and the environment. In considering management alternatives, I endorse the Forest Service's goal of cooperation and understanding with the public rather than adversarial confrontation.

One limitation of the overall approach of this project is that generic alternatives are being discussed while specific strategies will be implemented. By this I mean that different people will have different interpretations of the same general guidelines set forth in a given alternative. While each may be undertaking a strategy that they, in all honesty, believe is dictated by the document, others may disagree. My comments correspond to my best interpretation of the alternatives as set forth in the DEIS. And yet, I can see how implementation within the guidelines could undermine the very principles that make the alternative attractive to me. The lack of specificity is a concern which mandates strict monitoring of the strategies being implemented.

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Chapter II: The Alternatives

The Forest Service has identified 4 management strategies and 5 mitigation measures. These are combined into 7 alternative policies for management of forest resources. The management strategies are: prevention, maintenance, correction and no action. The five mitigation measures are: biological control, prescribed burning, manual control, mechanical control and herbicide use. These are combined as follows:

- A. No herbicides, other strategies okay.
Action begins at first sign of potential damage, before damage occurs
Prevention and correction ok. No herbicides
This is a continuation of current policy (since 1984). Can respond to seeds and seedlings.
- B. All effective tools can be used.
Action begins at first sign of potential damage, before damage occurs
Prevention and correction ok. Herbicides after Forest Service and EPA approval. All tools available.
- C. Action implemented rarely and only for human safety reasons.
Correction only. No fire or herbicides.
- D. Action with goal of prevention and in concert with natural processes.
Implementation at first clear sign of potentially significant damage
Prevention preferred; herbicides as last option.
This is an IPM type alternative. Requires surveillance and monitoring. Vegetation is managed to avoid need for corrective actions. Considers the health of the ecosystem.
- E. Action with herbicide use restricted and special worker safety provisions implemented. Use of certain pesticides prohibited and no aerial spraying.
Implementation at first sign before significant damage occurs. Prevention preferred.
No burning of herbicide treated vegetation. No use of chemicals in municipal or industrial watersheds.
- F. All methods for management are available with the exception of burning for silviculture.
Action begins at first sign before damage occurs
Prevention and correction methods recommended. No fire to treat slash or for planting.
- G. All methods for management are available with the exception of burning for silviculture.
Action begins at first sign, before damage occurs
Prevention and correction
The plan allows for much lee-way and judgment by the manager. Implementation of this approach implies aggressive management.

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The Forest Service prefers alternatives B, D and E. They would like to have the opportunity for herbicide use and burning, although they respect the concerns for human health, environmental damage and efficient ecological management. The options under these alternatives restrict their flexibility but do not prohibit their use of any tool. I concur with selection of alternative D and E. However, I qualify this by stipulating especial vigilance of the choice of herbicide for a particular problem based on human health and environmental risk and limitation of the use of herbicides whenever possible.

Comments on Chapter II

Overall, the problem seems to be the identification of an effective management policy for preserving forest resources while balancing the risk to human health, the risks to the environment and the cost of the program.

Forest workers, as workers in any industry, represent a small population that likely will be exposed to greater hazard than the public at large. Due to their routine higher exposures, they are at greatest risk. The public may be exposed via routine treatments and via accidents.

In considering Quantitative Risk Assessment (QRA) as a tool in setting management priorities, one must recognize its limitations. QRA is a methodology designed to evaluate the consequences of a variety of likely outcomes. For events that can occur routinely, the methods result in a set of numbers indicating the relative frequency and severity of harm. The methods, however, are notoriously unreliable for catastrophic and unusual events. Two well known examples of the inability of scientists to forecast events and risks using QRA are the nuclear accident at Three Mile Island and the gas leak in Bhopal, India. Both of these events had negligible risks as evaluated by QRA and yet each occurred (with a certainty of 1). While there is not a reliable alternative, this absence does not validate QRA. Results of all QRAs should be interpreted with caution. In considering the QRAs provided in the DEIS, one must recognize the limitations in terms of accidents and catastrophic events. Additionally, one must recognize that the methodology is notoriously imprecise. Errors in exposure are compounded with errors in potency estimation and errors in the evaluation of population or individual sensitivity. Ideally, QRAs would report ranges of risks, providing both maximum individual risk

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incurred (under a worst case scenario) and total community or population risk (a map of risk multiplied by a map of population density). Thus, strategies could be chosen on the basis that they neither expose any individual to extraordinary risk nor the entire community under consideration to an unhealthy situation.

The table comparing the seven different strategies, Figure II-3, was very useful in evaluating the alternatives. Construction of a similar table for projected health effects would have been useful.

In estimating the risks between alternatives, the DEIS first estimates the number of acres treated by each management method for each alternative and then multiplies this number by an accident or exposure rate for this management activity. In estimating both the number of acres treated and the accident or exposure rate, no reference is given to any data. It would be useful to know where these numbers came from and how reliable they are. Are they based on true accident rates over the past 5 or 10 years in this region (as I hope they are), or a broader based accident record or are they just an intelligent guess. Without an answer to this question, it is impossible to judge the importance of the differences between the numbers in Tables II-1, II-2 and II-3. Without a basis to judge the adequacy of the exposure measures, the entire QRA is brought into question. (There is some discussion of similar data in later chapters, but the relevance to these tables is not stated explicitly.)

In assessing the risk of fires to the public and to workers, it would be useful to know what constituents are found in the smoke and how the constituents might vary under the different alternatives. For instance, if a forest is treated with 2,4-D and then a wildfire occurs, will the risk of inhaling this smoke be similar to the risk incurred from inhaling smoke from a prescribed fire 3 times as large (Forest Service multiplication factor) on untreated forest? Is the principle risk to human health the organics, the particulates or the acid gases? How much of the hazard comes from herbicides and how much from burning forest products alone? (Some of these questions are answered in Appendix D.5 but are not brought forward into Chapter II. Further, the authors acknowledge the inadequate database on p. IV-120.) Are there health effects from burning other than carcinogenicity? For the carcinogenicity estimates, NOELs are used for consideration of burn conditions. Given that the other analyses for carcinogenicity in the DEIS used a no threshold model, why do these analyses use NOELs? I suggest that these be recalculated to be directly comparable to the other risks of cancer. Other health risks should be established on a comparative basis. Consideration of susceptible individuals, such as those with asthma or other lung/breathing problems could be mentioned.

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Manual methods have some health risks but these are limited to workers. These risks include hazards of working on steep slopes, in dense brush, exposure to wildlife, accidents with power saws and inhalation of exhaust fumes from power saws.

Mechanical methods can lead to a variety of accidents involving large and complicated machinery. In addition, workers must be in the field and are exposed to the same hazards as in the manual methods. This is not spelled out in Chap. II and should be.

Herbicide use poses a variety of hazards for both workers and others in the forest. In addition, they pose serious risks to the environment and wildlife of the forest. The risks posed will be affected by the application procedure, be it aerial, mechanical, backpack application or hand application. Aerial application is preferred by the Forest Service in terms of cost efficiency and overall coverage. However, its problems are most severe including drift, migration, damage to desirable vegetation and worker hazard. Further, management of application, identification of buffer regions and environmental monitoring for contamination make this strategy more cumbersome. Mechanical application removes many of the problems of aerial application by being more location specific, but is not as cost effective. To mitigate problems, the Forest Service proposes to notify downstream water users and land owners prior to application, to follow state and federal pesticide use guidelines and labels and to exercise extreme care in mixing herbicides and cleaning equipment. While these precautions help limit exposure they are not sufficient to prevent it. Buffers will be required near waterways and sprayers will be shut off over waterways. An evaluation of groundwater flow and would be useful to evaluation this. Pesticide manufacturers have underestimated this problem routinely and EPA has taken a lax stand towards requiring more accurate and extensive information. This is a serious risk that cannot be evaluated adequately due to unacceptable data quality. More environmental fate data should be required before implementation.

The summary table, Fig II-10, is very useful.

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Chapter III: The Affected Environment

This chapter describes the region that will be affected by the proposed EIS alternative. The report cites the limited information available on non-target species and recommends further study. I concur and again point out that evaluation of the risk is not possible without this information. Degradation of the fauna and flora can have disastrous effects on the structure and functioning of natural ecosystems. Some evaluation of the species richness (diversity) and its anticipated changes should be included in this document. Wildlife habitat availability and destruction should be considered in more detail. What are the implications of the alternatives with respect to the stability and resilience of the ecosystem? It is good that "all the alternatives specify ways to avoid, reduce, minimize and rectify potential adverse impacts." I hope that adequate measures will be taken to see that they are always implemented.

Chapter IV: Environmental Consequences

This chapter discusses the environmental consequences of implementation of the various management strategies. It discusses estimated impact and the uncertainty in those estimates. Comparisons are made with respect to Alternative B.

While the introduction to this chapter notes that incomplete or unavailable information is summarized in Appendix H, this does not obviate the need to consider effects not reported on in deriving an overall evaluation. To the degree that information is not available or is incomplete, the evaluation of risks is inadequate. This is not a fault of the investigators but rather a limitation of the applicability of this report.

Human Health Effects

This discussion is limited to 16 herbicides. ORA methods were used to estimate risks, and the quality of the information being used to make those estimates was evaluated qualitatively. Data were reviewed for general systemic toxicity, mutagenicity, carcinogenicity, reproductive toxicity, developmental toxicity, neurotoxicity and

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immunotoxicity. The data came from epidemiological studies, animal bioassays, short-term assays and comparisons of molecular structure. Uncertainties derived in Appendix H are listed in this chapter, but not incorporated into any of the quantitative evaluations.

The data evaluation included NOELs for general systemic toxicity, reproduction toxicity and development toxicity. Carcinogenicity is modeled using LOELs. The assumption of these approach is that there is an identifiable and consistent threshold among test organisms. No justification for this selection of models and assumptions is given. The assumption for carcinogenicity is less conservative than that for the other outcomes. This is not stated explicitly in the report and no justification is given. More detailed criticism is provided below in the section on Safety Factors. In Appendix D, more traditional dose-response modeling is used with a one-hit model. This has limitations, as well, that are detailed in the QRA literature. Most notably, this model has no biological component and is purely statistical.

Exposure evaluation was conducted by considering environmental transport and subsequent route of exposure. Backpack applicator exposures are assumed to be 15 days per year for 40 years of a 70 year lifetime. Resident exposure is characterized as "routine-realistic" exposure once a year for 40 years and one accidental exposure over a 70 year lifetime. Justification for these numbers are representative and conservative would be useful.

Safety Factors

On p. IV-94, Ames et al. (1987) concept of HERPs is discussed. Essentially, they use TD_{50} 's as their index of toxic potential in assessing the potency of chemicals. It is difficult to imagine how this index would be useful or conservative (in terms of health). In statistical terms, the TD_{50} is an index of central tendency. It is that dose at which 50% of the test animals got tumors. It is the median dose in terms of tumor response. As a median, it is a robust index of central tendency insensitive to subtle differences among chemicals. These subtle differences may be of paramount important in assessing the impact of exposure to large populations, especially at low concentrations, the situations we are most concerned with in this report. It gives no consideration to the shape of the underlying distribution, which may vary among chemicals, or the spread (variance) of the distribution. Consider a population with a normal distribution of outcomes, a central tendency of 10 (along an

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arbitrary index scale) and a variance of 1. Only one in 100 times will we see a value less than 7.64 or higher than 12.36. If the same population had a variance of 0.01, one would see a value less than 9.764 or greater than 10.236 only 1 in 100 times. Further, if we consider a third chemical with a lognormal distribution, the results would be still different. Yet, in all cases, the central tendency is the same. The TD_{50} s are the same. Yet, the responses at low concentrations are different.

The TD_{50} concept is very rigid and has a series of implicit distributional assumptions. The lack of allowance for different variances among responses to different chemicals, which might be indicative of different biological response mechanisms, is a major drawback. Risk assessors are just starting to build some biology into risk assessment methodology by using physiologically-based pharmacokinetic (PBPK) models in the estimation of cancer potency. The HERP approach takes a step in the opposite direction, removing biological considerations for potency evaluation and must be viewed skeptically. We must use additional information in the comparison of chemicals of unknown effects and potencies. (I was unable to find the further discussion of this topic in Appendix H that was promised on the bottom of p. IV-94.)

Carcinogenicity

The discussion of carcinogenicity of 2,4-D and 2,4-DP is appropriately non-committal and tentative. The conclusions of several studies are reported fairly as are their limitations. There is suggestion of carcinogenicity.

The Summary of Toxic Effects

The summary of toxic effects for the 16 herbicides is a useful compilation of a complex database. I am concerned about the paucity of data on neurologic and immunologic effects. Although I recognize that these are fairly new areas of investigation, the absence of information does not allow one to rule out the possibility of serious hazard. Data on other types of toxicity is better, although in Table IV-23 of 96 types of studies only 13 are rated as adequate. This suggests a tremendous gap in necessary information that is required to protect

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public health. For those situations that do not have adequate data one must interpret the toxicological information with appropriate uncertainty considerations. As noted above, I am very skeptical of the HERP LOELs in Table IV-27.

Environmental effects studies listed in Table IV-22 are better represented than the human health effects studies, although only 2 herbicides have adequate animal cancer data. The great lack of data suggests that one is not justified in drawing any but the weakest conclusions from these data. Given this situation, I am not sure how Tables IV-24 and IV-25 were constructed. I am also not sure how you could be more certain of no adverse affects from routine worker exposure than from routine public exposure (i.e., amitrole-developmental toxicity). Finally, all calculations in this section are dependent on exposure assumptions which themselves are suspect.

Considerations of inerts, interactions and impurities are critical in developing a thorough risk profile. As noted in the report, there is insufficient data to do so. Again, this undermines the validity of the risk assessment. Data must be obtained for these situations to adequately assess human and environmental hazard.

Appendix D: Human Health Risk Assessment (Quantitative)

My comments on this appendix are somewhat cursory as my attention was directed by USDA at other sections. In the introduction, the authors make the interesting point that human health response may vary depending on whether exposure is acute or chronic. Certainly, this makes sense. However, it is not incorporated into the evaluations in a quantitative manner.

The exposure evaluations provided are very limited. The authors claim that they are a worst case scenario, but certainly more adverse conditions could be imagined. No justification is given for the claim of worst case. There should be some qualification of the statements in this appendix.

The one-hit model for low dose extrapolation is used in the DEIS. This is surprising in light of EPA's recommendation of using the linearized multistage model. More discussion of this choice and the impact of using alternative models should be presented. A discussion of the variability in low dose response estimation based on the choice of model would be useful in characterizing the variability and unreliability in risk estimates.

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and in supporting the notion of comparative rather than absolute evaluation of risk. Some investigators have shown that some chemicals have supralinear dose response curves. In such an eventuality the one hit model is markedly non-conservative.

The exposure section of this document is the weakest point of the entire analysis. Use of terms like "routine-worst case" suggest inappropriate considerations of adverse situations. If something is routine, it cannot be worst case. The scenarios appear to be meant to portray adverse conditions but then are mollified so as not to be too bad. Certainly, worst case and accidental situations should be given ample consideration. Public exposure estimates are not based on field studies but rather on modeling exercises. Again, are these likely to lead to "worst" cases? I doubt it.

The use of a safety factor of 100 is suspect. While this number is used routinely in toxicology, it is explicitly assumed that this number represents a worst case for all chemicals. But, chemicals with steep dose response curves ought to be modeled with even larger safety factors (1000, 10000, etc.) to prevent accidental problems. If a single safety factor is used instead of a complete dose response curve, one must allow for an ample margin of safety for all chemicals in all situations. One must allow for variations among chemicals and for differential sensitivities to different agents. A safety factor of 100 does not provide this level of assurance. I also doubt that this factor accommodates the most sensitive subpopulation. Risk assessments should.

Risk comparisons of herbicide use with electrocution, motor vehicle accidents and like (Table 5-15) is wholly inappropriate. Risks among alternatives should be compared, but these risks should not be compared with totally unrelated activities. The choice confronting forest managers is whether to use herbicides or other management options. Knowing that herbicide use is safer than skydiving begs the issue of how best to manage a forest.

Appendix H: Human Health Risk Assessment (Qualitative)

The goals set out in this chapter are very innovative and worthwhile. Noting the controversies surrounding the interpretation of human health risk estimates, the uncertainties and judgments inherent in the QRA process as discussed above, and variations in the quality and completeness of the data on which to base these judgments, the authors chose to review the quality, consistency and completeness of the data.

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The first observation presented is that in a study of 770 chemicals, researchers found that the TD_{50} (a fairly robust index of cancer potency) varied about 7 orders of magnitude for all chemicals and about 3 orders of magnitude for some individual chemicals. They acknowledge that these variations may be due, in part, to animal species differences, methods of administration of the chemicals, and other factors. However, they offer no advice as to how this information should be used, how it might impact QRA, what adjustments might be made, etc. Again, given this great degree of variation, is it reasonable to assume that population responses to all these chemicals have identical distributional forms and equal variances? If not, one ought not use TD_{50} s.

While resolving these issues of uncertainty is the apparent goal of this section, a more explicit statement of goals, assumptions and procedures would be useful. While the evaluation is qualitative, one still uses rules in making qualitative decisions and an explicit statement of these rules would enable readers to evaluate the way in which the authors have made their judgments.

For example, the authors do not share their opinions as to the interpretation and reliability of these data. Are we to believe that these are statistical fluctuations that encompass the true range of biological variability, are the experiments so sensitive that even under the best of conditions we cannot expect to avoid this kind of variation, or are there other explanations to the wide range of observed responses? How are we to interpret these numbers in an effort to protect human health? Should we adopt an extremely conservative view and assume worst case, that is, the worst result? Should we try to develop a statistical model that seeks the most parsimonious solution (a Bayesian view)? Should we seek the region of greatest agreement and disregard all other values? Some statement should be provided.

In evaluating these data, the authors first review statistical issues of false positives, false negatives, significance and power. This is useful albeit somewhat divorced from the evaluation they conduct later. One issue that they fail to address is the effects of multiple comparisons. The errors they discuss reflect problems in a single evaluation, a test-based error. In fact, in the evaluation of many statistical tests one must be concerned about the combined error rate for all tests. If one performs the same test 20 times one would expect to get one significant result ($p = 0.05$) even if effect does not exist. In examining a data set as vast as the one described, one

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would expect many such false positives. Methods exist to adjust for the multiple comparison problem, but they tend to be very conservative. Thus, I do not recommend such adjustments but think that the problem should at least be mentioned.

The authors continue to discuss issues of false error rates and their interpretation. They point out the limitations of the analytic methods both in terms of classifying a dangerous chemical as not toxic (failure to consider alternative management strategies) and in terms of falsely classifying a material as toxic (failure to use a particular chemical). What is difficult with this problem is the reliability of multiple tests. How can one weight the validity of a set of test results for a particular chemical? They note the conservatism in using the upper 95% confidence interval for a 50 animal test of a substance with no effect and the weak power of such a test with a substance that produces excess risk. These observations point up the weakness of our methodology and the concern with which they should be interpreted.

Finally, the researchers note problems with extrapolations between species and from high dose to low dose. Further problems arise for consideration of interactions and complex mixtures, health endpoints for which standard protocols do not exist and the lack of attention to hyper-sensitive individuals.

Data problems that exist include data gaps for active ingredients covered by the regulatory testing programs and lack of tests for other compounds ("inerts") in the formulation of the herbicides. The authors note these problems but do not state how they will be accommodated in their qualitative risk assessment. I am particularly troubled by lack of information. For instance, with inert materials, since we do not even know what they are and have not tested them for toxic potential I do not see how they can be considered as part of the risk evaluation.

This section proceeds with an evaluation of the lowest NOEL as an index of chronic toxicity for the 16 herbicides under consideration. The data were judged for consistency and quality (i.e., thoroughness, duration, multiple species, quantitative agreement). No mention is made of how the statistical problems discussed in the previous section will be addressed. This is particularly troublesome in terms of the discussion of statistical power.

Consider the following situation. Chemical A does indeed cause an effect. The threshold varies among subjects but has a mean at threshold T with a specified variance. (This is the traditional QRA model.) One test

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is run with 30 animals at 5 doses and at the lowest non-zero dose no animal shows an effect. The study is rerun with 300 hundred animals and, due to the increased sample size an effect at all non-zero doses is detected in at least one animal. The effect is real. The different in experimental results is attributable to sample size alone. How should these numbers be viewed. Some guidance is needed.

Review of the October, 1987 Pacific Northwest
Region Draft Environmental Impact Statement
for Managing Competing and Unwanted Vegetation

by

Francis W. Weir, Ph.D., CIH, DABT

Houston, Texas

January, 1988

In accordance with a request of the United States Department of Agriculture Forest Service under purchase order number 40-4H1-8-0601, the author has reviewed the subject draft Environmental Impact Statement (EIS) and the accompanying appendices in regard to:

1. How well the document characterizes human health risk,
2. The validity of human health risk conclusions drawn in the draft Impact Statement,
3. How well the document incorporates considerations of human health risk into planning,
4. Recommendations of different possibilities for risk management for both workers and the public, and
5. Any additional points or comments that might materially affect the interpretation or use of the health risk information in the draft EIS.

General Comments.

This draft EIS presents an extraordinary quantity of information regarding the subject. There is a wealth of data regarding the Oregon and Washington forests and the host of problems involved with managing the complications of such a diverse system. The technically oriented reader is left with the impression that considerable attempt was made by the authors to present a comprehensive review.

On the other hand, the stated purpose of the EIS is to assure that the goals of the National Environmental Policy Act (NEPA) are met. These goals are to make all pertinent environmental information and analyses available to public officials, cooperating agencies and the public prior to formulating any decisions for action (Chapter I, page 14).

The Forest Service apparently has a further goal for the EIS in regard to human health issues:

"...To create an environment of cooperation and understanding rather than an adversarial situation." (Chapter I, page 8)

In regard to both the NEPA and Forest Service goals, in many areas this draft EIS is overly detailed to the extent that the "pertinent environmental information and analyses" are buried. In this reviewer's experience, public officials and the public are wary and distrustful of extensive detail. They equate complexity with obscurantism, and, as quoted in the draft EIS from earlier public comments:

"The heart of the issue is TRUST." (Chapter I, page 8)

Both groups need:

- a succinct presentation of the issues,
- the extent and limitations of available information,
- actions to be taken, and
- the rationale for each action under consideration.

Therefore, in preparation of the final version of the EIS, every effort should be made to present the material in a more succinct manner and to place information that is not absolutely critical to the development of each subject into an appropriately referenced appendix.

Comments regarding how well the document characterizes human health risk.

The draft EIS correctly assesses that concerns for human health should primarily focus on the safety of herbicides, with a secondary focus on smoke from prescribed burning. These two areas not only have the greatest potential for directly affecting human health but also are perceived as health concerns by the public.

Unfortunately, in regard to the safety of herbicides, throughout Appendix D there are statements reflecting that toxicity, exposure and risk were consistently overstated by the authors of the draft EIS presumably in an effort to be conservative. Examples of such statements include (but are not limited to):

"This risk assessment uses the MOS approach discussed above in comparing one-time human doses to lifetime animal doses in all of these cases even though this leads to an exaggeration of the risks." (Appendix D, Section 1, page 2, paragraph 2)

"A third area of uncertainty involves the estimation of the human doses liable to occur in herbicide use. This risk assessment has been designed to overestimate doses to err on the side of safety." (Appendix D, Section 1, page 2, paragraph 4)

"In addition, no member of the public is likely to receive as high a dose as estimated in this risk assessment..." (ibid.)

"Furthermore, the public doses estimated here exaggerate the amount they could receive." (ibid.)

"Thus, the way in which exposures are estimated in this risk assessment and the way the risks are judged both tend to exaggerate the real risks." (ibid.)

"The analysis showed that currently there is scientific uncertainty regarding the potential of five of the herbicides...to cause cancer in humans. Therefore, this risk analysis uses the worst case assumption that these five herbicides would cause cancer in exposed persons." (Appendix D, Section 1, page 8, paragraph 3)

[In regard to resident exposure], "A combination of factors makes the possibility of the resident receiving such a dose highly unlikely." (Appendix D, Section 4, page 7, paragraph 4)

[In regard to worker exposure]"...the realistic dose estimates are higher than those that would occur in actual operations..." (Appendix D, Section 4, page 12, paragraph 2)

There are also a number of examples where data were selected from studies to purposely exaggerate the toxicity. For instance, in regard to cancer:

"In cases where there is more than one data set available, the data set indicating greater carcinogen potency has been chosen." (Appendix D, Section 5, page 21, paragraph 6)

Further, in regard to the health risk resulting from exposure to diesel oil, presumably to be used as a herbicide diluent;

"Diesel oil has not been shown to be carcinogenic, but...For the purpose of this risk assessment, diesel oil is assumed to be carcinogenic..." (Appendix D, Section 3, page 30, paragraph 1)

Such a practice of indiscriminately overestimating the toxicity, the potential for exposure and the resulting risk might be laudable from a humanitarian perspective, but is not scientifically acceptable. This is especially true since there appears to be no way for the reader of the EIS to quantitatively measure the degree of overestimation. Further, the less critical reader of the EIS document, (i.e. the public official and/or the public) is not clearly informed regarding either the specific use of overestimation or the cumulative effect of the overestimates of toxicity and exposure on overstating the risk.

The authors of the EIS should re-evaluate their apparent policy of arbitrarily overestimating both the toxicity and exposure in order to provide both decision makers and the concerned public with a more realistic appraisal of the risk. Alternatively, all the areas of overestimation that have any impact on the various risk estimate should be specifically and clearly presented in all applicable sections of the EIS. If this alternative is selected by the author of the final EIS, to the extent feasible, a quantitative estimate of any overestimation should be provided so that the EIS reader can make judgments with proper information.

Also, the authors of the draft EIS have made assumptions regarding the carcinogenicity of the 16 herbicides considered that appear to be less than consistent with the interpretation of other interested national and international organizations. Apparently, eight of the 16 herbicides under consideration within this draft EIS are estimated to have some human cancer potential (see Appendix D, Section 3, pages 23-27).

The American Conference of Governmental Industrial Hygienists (ACGIH) addresses the human health hazard from six of the candidate materials including 2,4-D, Picloram, Atrazine, Diuron, Amitrole and Bromacil. The ACGIH, however, does not consider that any of these materials represent a human carcinogenic hazard at least in regard to worker exposure. (1)

The International Agency for Research in Cancer (IARC) in their 1982 listing, only recognizes Amitrole as a carcinogen. IARC indicates that Amitrole is a 2B carcinogen; thus indicating that it is a probable human carcinogen because of sufficient evidence in animals but inadequate human evidence. (2)

The National Toxicology Program 1985 listing of human carcinogens lists only Amitrole as a carcinogen. (3)

The RTECS cumulative supplement for 1983-84 compiled by the National Institute of Occupational Safety and Health (NIOSH), lists Picloram and Amitrole as carcinogens based on animal studies. (4)

The Occupational Safety and Health Administration (OSHA) lists a Permissible Exposure Level (PEL) only for 2,4-D. However, they do not recognize this material as a human carcinogen of interest in the workplace.

1. Threshold Limit Values and Biological Exposure Indices for 1987-88, American Conference of Governmental Industrial Hygienists

2. IARC (1982) Monographs on the Evaluation of the Carcinogenic Risk of Chemicals to Humans, Suppl. 4, Chemicals, Industrial Processes and Industries Associated with Cancer in Humans, IARC Monographs, Volumes 1 to 29, International Agency for Research on Cancer, Lyon

3. Fourth Annual Report on Carcinogens, Summary 1985, U. S. Department of Health and Human Services, Public Health Service

4. Registry of Toxic Effects of Chemical Substances, 1983-84 Supplement, U. S. Department of Health and Human Services, Public Health Service

For health effects from herbicides other than carcinogenicity, the draft EIS states that:

"...exposure of any member of the public from any of these scenarios is not probable: for those individuals who do get exposed, it is not likely to occur more than once or twice.

"Public exposure from accidental spraying is less likely, and would not be expected to occur more than once to any individual."
(Chapter 4, pages 91-92)

The evidence available concerning the very low toxicity of these materials of concern combined with the above suggestions of very low exposure potential should be ample documentation to limit further concern.

In regard to human health effects related to burning, either prescribed or accidental (i.e. wildfire), the draft EIS correctly assesses that particulate emissions represent the primary concern.

The draft EIS indicates that considerable information is either incomplete or unavailable for human health effects of smoke associated with the management of vegetation, slash burning or wildfire. The authors indicate further that the state of the art for evaluating air toxics is relatively primitive, compared to the toxicology of pesticides. They specifically state that information is incomplete or unavailable for:

"-characterization of human exposures to smoke, including composition of smoke; concentration of its components; duration of exposures; and populations exposed;

-The toxicology of many of the constituents of smoke; and

-Methodologies...for characterizing human health risks from smoke from forest-prescribed burns and wildfires." (Chapter IV, pages 9-10)

Much of the above is precisely true, however, there is considerable information regarding the toxicity and hazard from the principal emissions (Chapter IV, page 39) including:

- carbon monoxide
- nitrogen oxides
- hydrocarbons of interest, and
- particulates.

For the most part, there is adequate information available regarding the toxicity and probable human health risk from likely exposures to all materials mentioned above except for the particulates.

While most of this information cannot be obtained from circumstances precisely similar to those from "forest-prescribed burns and wildfires", the knowledge base is not as sparse as the author of the EIS presented in Chapter IV, page 10, paragraph 1. Although there appears to be little or no reason for human health concern from likely public exposure to these materials, to the extent the authors of the EIS have interest, the published health effects literature regarding these materials is widely available.

In regard to particulates, the authors of the draft EIS indicate that most studies of particulate matter have been associated with concurrent exposures to one or more sulfur oxides. However, sulfur oxides are generally not considered to be an important factor in regard to forest-fire derived particulate. A rational reviewer can only interpret that specific toxicity of the sulfur-oxide free particulate of concern in the forest-fire smoke consideration is minimal, perhaps approaching that of nuisance particulate. Again, although there is less than adequate information regarding the specific materials of concern, there is considerable information in the literature regarding the hazard potential for non-specific nuisance particulates as well as information concerning exposure levels of such particulates to various urban populations.

There is also some substantial indirect evidence that the total human health impact from forest-fire emissions may not be remarkable. This evidence is presented in the draft EIS wherein the authors indicate that on the one hand, fire-derived smoke has been an important air contaminant in the region for many years, and on the other hand, that the public health status in the region is better than it is for the U.S. overall (Chapter III, page 46). This more favorable health status appears to be true for all deaths as well as for cancer deaths.

Thus, in response to the question of how well the document characterizes human health risks, it is apparent that the October 1987 draft EIS grossly overstates human health risks by:

- overconservatively interpreting the results of toxicity information,
- overestimating the potential for exposures, and
- not significantly utilizing available supporting information to augment health risk documentation.

Comments regarding the validity of human health risk conclusions drawn in the draft impact statement.

Drawing from the material presented above, it is obvious that the authors of the draft EIS have overstated the hazard and therefore the risks associated with use of either herbicides or resulting from exposure to smoke. In some cases, they have apparently not chosen to utilize documentation available from other organizations (e.g. in regard to cancer). In other cases, there is supporting documentation from similar circumstances that might be better utilized in arriving at an estimation of risk.

The authors of the draft EIS state that there is:

"...potential for reasonably foreseeable significant adverse impacts on the human environment. The potential impacts are in the areas of human health,..., and environmental effects (Chapter IV, page 7).

If the above represents the conclusion of the authors of the draft EIS, this reviewer must conclude that these authors have materially overstated their case. With the incorporation of appropriate controls to be described below, this reviewer concludes that adverse human health effect can be minimal and that there is low probability for a significant adverse impact on the human environment from judicious use of herbicides or prescribed fire.

Comments regarding how well the document incorporates considerations of human health risk into planning.

There is a general appreciation within toxicology that:

"All substances are poisons; there is none which is not a poison. The right dose differentiates a poison and a remedy."

Paracelsus
(1493-1541)

This statement and an understanding that while toxicity represents an inherent property of a material, hazard is dependent on circumstances. These considerations form the bases for chemical hazard control in both the industrial and general environment.

The informed use of forest-worker training and appropriate personal protective equipment will minimize worker health risks from any herbicide application from any of the alternatives described in the draft EIS. It was not clear from review of the draft EIS that these basic premises of Industrial Hygiene were adequately understood by the authors.

Likewise, in regard to public exposure, it is not clear that the authors adequately considered the probable use pattern of herbicides either when assessing risk or in planning. In Chapter III, page 47, they clearly state that two materials (2,4-D and Glyphosate) are likely to account for approximately 70% of all herbicide use. All other herbicide materials are expected to approximate 5% or less of use for each material. This use pattern apparently was not considered in assessing the risks of various alternatives. If on the one hand, these two major use materials (2,4-D and Glyphosate) represent a lesser human health risk than the other herbicides, then risk assessments involving use of herbicides should be lowered. Of course, the opposite could also be true. In any event, these considerations should be incorporated into planning.

In regard to the use of prescribed fire, it appears that the authors of the draft EIS have a good appreciation for procedures that should manage any impact from emissions on the general population. In this matter, there appears to be ample understanding of the principals and an appreciation for mechanisms to properly apply these principals.

Comments regarding recommendations of different possibilities for risk management for both workers and the public and any additional points or comments that might materially affect the interpretation or use of the health risk information in the draft EIS.

In regard to recommendations for different possibilities for risk management and any additional useful comments to be considered, response to these have been incorporated within the text of other sections of this review. Further discussion would be redundant.

CURRICULUM VITAE

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VITAL STATISTICS

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PRESENT POSITION

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EDUCATIONAL BACKGROUND AND DEGREES

1968	Doctor of Philosophy (Ph.D.) in Toxicology and Comparative Pharmacology University of California Medical Center San Francisco, California
1967	Master of Science (M.S.) in Pharmacology University of California Medical Center San Francisco, California
1957	Bachelor of Science (B.S.) in Zoology University of Pittsburgh Pittsburgh, Pennsylvania

CERTIFICATIONS

1980	Certified in the Comprehensive Practice of Industrial Hygiene (C.I.H.) by the American Board of Industrial Hygiene
1981	Certified in General Toxicology (D.A.B.T.) by the American Board of Toxicology

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ACADEMIC AND RESEARCH APPOINTMENTS

1982-1983	Research Associate Professor of Toxicology University of Texas School of Public Health, Houston, Texas
1976-1982	Associate Professor Toxicology University of Texas School of Public Health, Houston, Texas
1973-1976	Senior Research Toxicologist Biomedical Sciences Department, Research Laboratories, General Motors Corporation, Warren, Michigan
1969-1973	Assistant Professor of Toxicology Department of Preventive Medicine College of Medicine, Ohio State University, Columbus, Ohio
1962-1967	Research Toxicologist The Hine Laboratories, Inc. San Francisco, California
1958-1962	Biological Science Technician and Biologist (Toxicology), Toxicology Division, Medical Research Directorate Edgewood Arsenal, Maryland
1957-1958	Research Paint Chemist Pruett-Schaffer Chemical Company Pittsburgh, Pennsylvania

GRADUATE TEACHING RESPONSIBILITIES

1969-1973	Course Director: Environmental Toxicology (3 quarter sequence) to Graduate Students and Medical Residents in Occupational Health and Aerospace Medicine
1971-1973	Course Director: Environmental Health to Graduate Students in Preventive Medicine
1976-1982	Course Director: Principles of Toxicology (PH-428) to Graduate Students in Public Health

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GRADUATE TEACHING RESPONSIBILITIES (continued)

1977-1982	Course Director: Sites and Mechanisms of Toxicity (PH-429) to Graduate Students in Public Health
1976-1983	Co-Director, Lecturer: Chemical Contamination of the Environment, Environmental Health/Occupational Health, the Bases of Community Health, Principles of Industrial Hygiene, Control of Health Hazards in Foods and UTMS-H Selective in Occupational Medicine

OTHER APPOINTMENTS AND CONSULTANTSHIPS

1969-	Consultant on Health Effects of Chemicals General Motors Corporation, Detroit, Michigan
1970-1973	Member, State of Ohio Pesticide Advisory Committee Ohio State University, Columbus, Ohio
1971	Consultant on Health Effects of Sulfur Oxides Environmental Protection Agency State of New Mexico, Santa Fe, New Mexico
1971-1974	Consultant on Health Effects of Air Pollutants Ohio Electric Utilities Institute Columbus, Ohio
1971-1974	Consultant in Industrial Toxicants Community Laboratories of Ohio, Columbus, Ohio
1971-1973	Toxicologist Consultant for Development of State of Ohio, OSHA Legislation, Labor Education and Research Service, College of Law, Ohio State University, Columbus, Ohio
1972	Consultant on Industrial Lead Hazards Becton-Dickenson Company, Kent, Ohio
1972-1973	Consultant on Health Effects of Air Pollutants Attorney General, State of Ohio, Columbus, Ohio
1973	Member, Review Committee on Sulfur Dioxide National Institute for Occupational Safety and Health, Washington, D.C.

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OTHER APPOINTMENTS AND CONSULTANTSHIPS (continued)

1973	Member, Review Committee on Sulfur Oxides National Institute of Environmental Health Sciences, Washington, D. C.
1974-1975	Member, Medical Advisory Committee Chairman, Sub-Committee on Carbon Monoxide Coordinating Research Council, Inc. New York, New York
1974-1975	Member, Committee on Epidemiological Studies Motor Vehicle Manufacturers Association Detroit, Michigan
1975-1976	Member, Technical Committee, Society of Toxicology
1976	Member, Extramural Research Evaluation Committee Electric Power Research Institute, Palo Alto, California
1976-1977	Consultant on Health Effects of Air Pollutants Greenfield Attaway and Tyler, Incorporated San Rafael, California
1976-1978	Consultant on Health Effects of Carbon Monoxide Liquid Carbonic Corporation, New Orleans, Louisiana
1977	Consultant on Health Effects of Sulfur Oxides Stauffer Chemical Company, Houston, Texas
1977	Consultant on Health Effects of Diesel Exhaust American Mining Congress, Washington, D. C.
1978	Consultant on Health Hazards from Industrial Exposure to Copper and Silver Dust American Smelting and Refining Company, Helena, Montana
1978-1980	Consultant on Hazards of Airborne Asbestos Houston Independent School District Houston, Texas
1978-1982	Consultant on Health Hazards of Closed Cycle Water Re-use Systems National Aeronautics and Space Administration Houston, Texas

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OTHER APPOINTMENTS AND CONSULTANTSHIPS (continued)

1979-1981	Member, Review Committee for Water Quality Criteria Document Program Environmental Protection Agency, Cincinnati, Ohio
1979-	Consultant in Continuing Education Program American Industrial Hygiene Association
1979	Consultant on Health Effects of Water Contaminants Champion Paper Company, Houston, Texas
1979	Consultant for Continuing Education Program in Industrial Toxicology Tenneco Corporation, Houston, Texas
1979	Consultant for Development of NIOSH Training Course in Industrial Toxicology Dunlap and Associates, Inc., Darien, Connecticut
1979	Consultant on Health Hazards of Diesel Exhaust, Engine Manufacturers Association Chicago, Illinois
1979-1982	Member, Scientific Advisory Committee Regarding Hazards of Solvents in Transportation for Southwest Research Institute, San Antonio, Texas under contract to the U.S. Coast Guard
1980-1982	Consultant in Industrial Toxicology, Atlantic Richfield Company, Houston, Texas
1980	Consultant regarding EPA Air Carcinogen Policy, American Mining Congress, Washington, D.C.
1980	Consultant in Health Effects of Acrylonitrile, Standard Oil Company of Ohio, Cleveland, Ohio
1980-1983	Consultant in Toxicology, Citizens Committee on Hazardous Waste Disposal, Calcasieu Parish, Lake Charles, Louisiana
1980-1981	Member, Subcommittee on Toxic Substances, Committee on Food Protection, American Public Health Association
1981	Member, Review Committee for the Iron Multi Media Water Quality Document, Environmental Protection Agency, Cincinnati, Ohio

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OTHER APPOINTMENTS AND CONSULTANTSHIPS (continued)

1981	Member, Scientific Advisory Committee Regarding Cyanide Hazard, Houston Department of Public Health
1981-1983	Consultant on Hazards of Air Contaminants, Conroe Independent School District, Conroe, Texas
1981-1983	Member, Advisory Committee Regarding Health Effects of Pydrin, Shell Development Company, Houston, Texas
1981	Consultant in Industrial Toxicology, Dupont Corporation, Orange, Texas
1982-	Consultant regarding newspaper printing ink cancer risk, National Association of Printing Ink Manufacturers, Harrison, New York.
1982-1983	Consultant in Toxicology for the Investigation of Indoor Air Quality in Manufactured Housing Conducted by UTSPH, in behalf of the Texas Department of Labor and Standards, Austin, Texas
1982-1983	Consultant on Health Effects of Carbon Monoxide, S.C. Johnson and Son, Inc., Racine, Washington
1982	Consultant regarding Siting Waste Management Facilities in the Galveston Bay Area: for the Gulf Coast Waste Disposal Authority
1982-	Consultant regarding Health Hazards of Formaldehyde, Champion Home Builders, Co., Detroit, Michigan
1983	Consultant regarding Calcasieu Parish Hazardous Waste Problem for the Environmental Control Commission of the State of Louisiana, Baton Rouge, Louisiana
1983	Consultant regarding Drinking Water Criteria on Silver, Office of Drinking Water, Environmental Protection Agency, Washington, D. C.
1984-	Consultant regarding Workplace Exposure Hazards, American Newspaper Publishers Association, Washington, D. C.

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OTHER APPOINTMENTS AND CONSULTANTSHIPS (continued)

1984-	Consultant on Health Effects of Copy Processing Chemicals, Savin Corporation, Stamford, Conn.
1984-	Consultant regarding Health Hazards of Anesthesia Equipment, Baylor College of Medicine, Houston, Texas
1984	Consultant regarding Health Effects of Chemical Waste Dump Odors, Monsanto Co., St. Louis, Missouri
1984	Consultant regarding the Toxicology of Reduced Sulfur Compounds, Chesapeake Corporation, West Point, Virginia
1985-	Consultant on Health Effects from Exposure to Industrial Chemicals, Shell Oil Company, Houston, Texas
1985-	Consultant regarding Health Effects of Asbestos Exposure, Norfolk and Southern Railroad, Roanoke, Virginia, Seaboard System Railroad, Savannah, Ga.
1985	Consultant in Toxicology, Houston Fire Department Hazardous Materials Response Team, Houston, Texas

PROFESSIONAL SOCIETY MEMBERSHIPS

American Academy of Industrial Hygiene
American Association for the Advancement of Science
American Conference of Governmental Industrial Hygienists
American Industrial Hygiene Association
New York Academy of Sciences
Sigma Xi
Society for Risk Analysis
Society of Toxicology
Air Pollution Control Association

RESEARCH INTERESTS

Industrial Toxicology - Industrial Hygiene
Inhalation Toxicology Methodology
Mechanisms of Pulmonary Irritant Activity
Extrapolation of Data from Animals to Man

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- Weir, F.W. "Toxicology of the Sulfur Oxides," Journal of Occupational Medicine, Vol. 21, pp. 281-284, 1979
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Weir, F.W. and V.L. Fabiano, "Re-Evaluation of the Role of Carbon Monoxide in Production or Aggravation of Cardiovascular Disease Processes," Journal of Occupational Medicine, Vol. 24, pp. 519-525, 1982

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Managing Competing and Unwanted Vegetation Draft EIS
Review of Carcinogenesis sections, particularly in the appendix

By
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Although I was asked only to review Appendix section 5, I find that this depends upon other sections and other sections refer to section 5. The gravest problem I find in my review is an inconsistency between sections. Accordingly I go to these sections and suggest changes and in particular rewording, to make them logically complete and consistent.

A. The study of carcinogenicity starts with the data in section 3 of the appendix, entitled "Hazard Analysis." This data is cast in a framework which sets the criteria for a risk analysis, and in some cases unnecessarily so. The major problems here are:

1. The definition of carcinogenic is not made, and only implied. On pages 17 et seq., various phrases are used. Table 3-4 is most precise.

"Oncogenic in 1/3 studies"

"Non oncogenic in 2 studies"

thereby correctly implying that the definition depends on the study, and its sensitivity. There is no absolute definition of a non-oncogenic chemical. One can merely say that it has not been found to be oncogenic (carcinogenic) in any study so far.

On the whole, these pages stick to this. But in the discussion of carcinogenic potency (I would prefer this to "cancer potency" on p. 24) the point is blurred. Also on p. 24, the wording for simazine and tebuthiuron could be improved. Instead of "Available evidence does not indicate that tebuthiuron is carcinogenic" a better phrasing would be "tebuthiuron has not been found to be carcinogenic in any study so far."

This is important, because it is later claimed that the cancer risk estimates are a worst case study. They are not. It is important to emphasize the respects in which they are not at the appropriate places. Thus under "carcinogenic (or cancer) potency" on p. 23 I would start with a disclaimer

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(see later in rewrite of this section) or else calculate an upper 95th percentile for carcinogenic potency for all chemicals, including those for which the potency value is not statistically significant (therefore declared not to be carcinogenic by EPA) because the chemical could be carcinogenic, and the potency this high, without it having been detected.

2. The figure 3-2 is bad, and misleading. As drawn it implies linear axes, yet if so, human doses are much lower than drawn and the data more spread out. There is also a point at zero dose (background cancers). I strongly suggest abandoning this figure or replacing it with a figure that has real data for a real chemical, including error bars on each point, and with the line being the best fit to the data, and a dotted line being the upper 95th percentile. This I illustrate later on in a rewrite of these pages.

3. The formula

$$- \ln (1 - P(d)) = a + (b \times d)$$

is not a suitable formula for finding the carcinogenic potency b . We see this by recasting and going to the limit $b \rightarrow 0$.

$$\begin{aligned} P(d) &= 1 - \exp[-(a+bd)] \\ &= 1 - \exp(-a) \exp(-bd) \\ &\rightarrow 1 - \exp(-a) (1-bd) \text{ as } d \rightarrow 0 \\ &\rightarrow (1 - e^{-a}) + bd \exp(-a) \text{ as } d \rightarrow 0. \end{aligned}$$

a does not in this formula represent the background tumor level, as stated in the text

$$1 - e^{-a} = a - a^2/2! + \dots$$

does represent the tumor level. b is not the potency; $b e^{-a}$ is.

The difference is small, and the values of b derived are only 20% or so from correct values. But this is not the formula used later in section 6, and it suggests a greater sloppiness in analysis than I believe is the case. A better formula is used by Crouch and Wilson (J. Tox. Env. Hlth. 1979) and is

$$P(d) = 1 - (1-a) \exp(-bd/(1-a))$$

This goes to $a + bd$ as $d \rightarrow 0$ and goes to 1 as $d \rightarrow \infty$.

4. It is unnecessary and misleading to use the phrase "one-hit model"

particularly here, and also anywhere in the EIS. The use implies that there is more biological meaning to the formula than is the case. In this section where you are discussing data, the closer you stick to data, and a simple formula describing it, the better.

5. The definition of potency on p. 23 is good. For some it is worth paraphrasing ("alternatively described as the slope of the assumed dose-response relationship"). This definition immediately leads to the next point, which is not stated but should be.

"Thus potency is, in general, a function of the dose. It must be emphasized that the potency is derived here from data at high dose levels, and is only applicable to low doses by assuming the applicability of the formula."

6. This would break 2 important concepts of sources of uncertainties that are confused in this EIS, the animal/man comparison, and the high/low dose extrapolations. In several places I suggest how to emphasize this distinction.

It is not stated on pages 25-27 but I assume that the quoted potencies are potencies calculated for the animals mentioned. Not the human potencies derived therefrom. If not, this should be changed and these two potencies made distinct. (In section 6, an interspecies factor K is explicitly used, and this would therefore be a suitable place to discuss potencies in humans). I would restate every time that the animal potency is meant. e.g. for Atrazine the last line on the section on p. 26 would become "The carcinogenic potency in rats using this model is calculated to be 0.03 per (mg/kg day)(USDA 1986) upper 95th percentile." [Note that here I also dropped the phrase "one-hit model."]

7. Although it is common to express doses as mg/kg/day, it is formally incorrect unless parentheses are applied.

mg/kg/day means mg/(kg/day)
→ mg day/kg.

Better is (mg/kg)/day or mg/kgday.

8. Reference might usefully be made to Zeise, Crouch and Wilson on Dose Response Relations, Env. Hlth Perspectives 73, 258 (1987), and both Crouch and Wilson, J. Tox. Env. Hlth. (1979) and K. Crump report to the EPA, 1979 on interspecies comparisons. I give more precise references later.

Explicit rewrite of pages 23-25. Section 3 appendix.

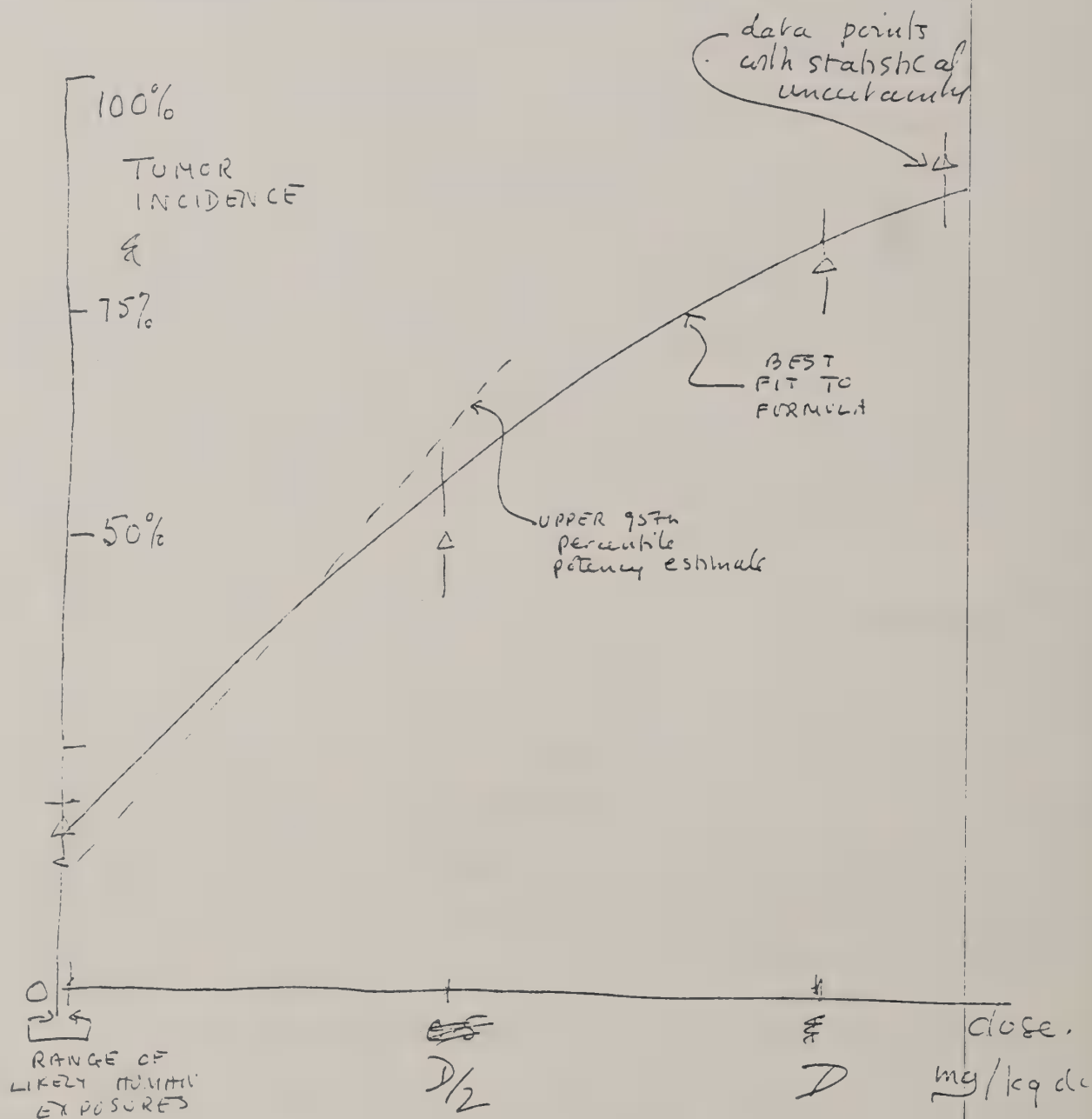
CARCINOGENIC POTENCY

This section presents the result of the carcinogenic potency calculation for the herbicides. The results will be the upper 95th percentile value calculated from a simple model. It is, in principle, possible to calculate an upper 95th percentile value from any set of data, including data which do not demonstrate a statistically significant increase in tumors, and which are referred to in the previous section as "available evidence does not indicate that X is carcinogenic" (or better: "X has not been found to be carcinogenic in any study so far"). This would represent a true "worst case." However, in this EIS we follow most Federal Agencies in ignoring the upper limit of carcinogenic potency in those cases where the study is deemed "adequate" and there is no statistical significance.

The carcinogenic potency of a chemical is defined as the increase in likelihood of getting cancer from a unit increase in the dose of the chemical. This may be equivalently defined as the slope of the dose-response relationship. This relationship is illustrated in figure 3-2. The data are the fractions of animals which got tumors at the dose level applied; the bars are the statistical errors on the points. The solid line is the best fit to the data using the simple formula below, and the dotted line is the upper 95th percentile value at low doses. The line specifies what the increase in cancer probability is for each increase in dose in mg/kg day.

The figure 3-2 shows that the dose levels used in the laboratory cancer studies (bioassays) are high, whereas those liable to be experienced by humans by exposure in the environment are low. The figure also shows that the carcinogenic potency is, in general, a function of applied dose. The potency is derived here from the data at high doses and its only applicable to the low

Figure 3-2
Data from bioassay on chemical X



[Data ~~are~~ here are
"typical". Put in
actual numbers]

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human doses within the limits of the applicability of the formula. For example, if the true carcinogenic response were to go to the background value at a threshold dose one tenth of that used in the bioassays, the potency derived from the data using the true dose response relationship would differ little from that derived here but the applicability to humans would be greatly different.

The following paragraphs give the specific sources of the carcinogenic potency estimates. Some of them were derived by EPA or by Crump using the Global 82 Computer Program (Crump 1983). The others were calculated directly from the data of the preceding section using a least squares regression procedure using the following formula:

$$-\ln [(1-P(d))/(1-a)] = (b \times d)/(1-a)$$

This formula can be rewritten

$$P(d) = 1 - (1-a) \exp - [bd/(1-a)]$$

from which it may be seen that at large doses ($d \rightarrow \infty$) $P(d) = 1$ and at small doses ($d \rightarrow 0$) $P(d) = a + bd$.

The slope estimated by the regression, b , (called β by some authors) is an estimate of the carcinogenic potency for the test animal. It should also be noted that the least squares estimates of a and b are [check here: also or approximately] maximum likelihood estimates using this formula. In all cases we used a 95-percent upper confidence limit for " b ". The parameter " a " is the background level of tumors in the test animal so that it does not enter into the calculations of the additional cancer risk due to the dose d . It should be noted that the Global 82 computer program reduces to this simple formula in the special case of only two dose values, or if there is no upward slope in the dose-response curve.

Several aspects of this analysis make the carcinogenic potency estimates very high (pessimistic). Firstly it is assumed that any dose, however small, has some finite though small probability of causing cancer. This is the non-threshold property of the formula, discussed previously, indicating that even

a single, small dose may be enough to trigger cancer. Some of the chemicals may have thresholds. Amitrole, for example, has been shown to cause cancer in test animals only at relatively high doses. EPA recommends using a threshold approach to analyze the carcinogenic risk posed by amitrole, but this analysis uses the non-threshold assumption for all seven herbicides.

Associated with this non-threshold assumption is the extrapolation from the high doses used in animal studies to the far lower doses that humans may get. Formulae other than the formula used here, which assumes a straight line at low doses as illustrated in figure 3-2 (and given by the low dose limit $a + bd$) have been used for extrapolation of data to assess human risk. These all have the property of the risk varying with dose at a higher rate than the linearity assumed by the formula. Although occasionally a formula yielding a response varying less rapidly than the dose (such as $P(d) = bd^{1/2}$) have been used (Food Safety Council 1978), these are not considered biologically plausible, and in this sense, the formula used here gives the highest estimate of cancer risk at the low doses liable to be seen in exposed humans.

Secondly the carcinogenic potency used in the calculations of human risk in this EIS is not the expected potency value, as given by the solid line in figure 3-2 but the dotted limit value of the 95 per cent confidence interval as shown in the dotted curve. Using this upper limit value gives a value of potency that varies from 30% higher to twice as high as the expected potency value.

One caveat needs to be stated here: there is at least one chemical (vinyl chloride) for which a simple application of the formula gives a potency too low at medium doses, because the fraction of animals getting cancer induced in Sprague-Dawley rats by inhaled vinyl chloride saturate at about 40%.

A full review of these issues, the theories, and the data on dose-response relationships may be found in Zeise, *et al.* (L. Zeise, E.A.C. Crouch and Richard Wilson, "Dose Response Relationships for Carcinogens: A Review" *Env. Hlth Perspectives* 73, 259, 1987).

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B. I now go to section 5 itself; the cancer risk part of it.

1. Again, avoid the phrase "one-hit model." e.g. p. 22, first line of section 4. P. 21 lines 24, 26 I prefer the use of the word formula rather than model to emphasize that the biological basis is slim.

I could say (p. 21, middle of page). "This would be true of the log-probit formula suggested by EPA (EPA 1984s) but instead a linear, no-threshold, formula has been used here under the assumptions that even low doses can cause cancer. The linear, no threshold, formula for estimating the risk for all herbicides in this analysis predicts the maximum rates of cancer that could occur at low doses under any of the formulae that have been in general use. Not surprisingly, at the high doses where the formulae are fitted to data, all formulae predict nearly the same rate of tumor formation.

There are two caveats to the above mentioned in section 3; (1) the other herbicides might cause cancer, although there is presently no evidence thereof; (2) some people have used formulae that predict higher rates than the linear no-threshold formula although these are not considered biologically plausible, and (3) the potency derived from the linear no-threshold formula of section 3 may be low if there is a saturation of tumor induction with dose, as happens with Sprague-Dawley rats inhaling vinyl chloride. These and other matters are discussed in detail in a recent review (Zeise, et al., 1987)."

Page 22 section 4

"The linear no-threshold formula was used..."

Page 23 section 5

Add: "For the cases where there are only 2 doses in the animal bioassay or there is no upward slope in the dose-response relationship, this computer program reduces to the simple, linear, no-threshold, model described in section 3 and drawn in figure 3-2."

Add in the middle of p. 24

Several of these assumptions give "conservatism factors" that multiply and cannot be distinguished, suggesting that risk might be overestimated by a factor between 10 and 100.

Several authors (Crouch and Wilson, 1979; Crump, et al., 1977) have compared (E.A.C. Crouch and Richard Wilson, "Interspecies Comparison of Carcinogenic Potency," J Tox. Env. Hlth, 1979 and K. Crump, et al., 1987 Report for EPA in press) human risks in cases where epidemiological data exists, and there are also animal bioassays. The best predictor of human risk comes from

- (a) averaging the animal studies and taking the most sensitive
- (b) assuming the "best value" of potency not the upper 95th percentile
- (c) assuming that carcinogenic potency in humans is equal to that in animals when doses are the same on a mg/kg day basis.

If we ascribe all the differences to carcinogenic potency we can state the uncertainty about this statement. For an individual the ratio of potency in man to that in animal varies from 1/20 to 20. This can be mathematically described as a value of σ , for a log normal distribution, of about 1.0 to 1.5.

The bottom of p. 24 and top of p. 25 needs modification and amplification. I do it here.

"The probability of occurrence of cancer in an animal is therefore given by the following equation already described in Section 3.

$$P(d) = 1 - (1-a) \exp[-bd/(1-a)]$$

At the doses of concern here, this reduces to

$$P(d) = a + bd$$

And neglecting the background tumors, the additional probability of tumors becomes

$$P(d) = bd$$

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The value of b was derived in section 3 for animals. The following specific values were used

amitrole	1.4 per (mg/kg day) in rats
2-4-DP	5.9×10^{-2} per (mg/kg day) in rats
asulam	2×10^{-2} per (mg/kg day) in rats
2-4-D	5.0×10^{-3} per (mg/kg day) in rats
bromacil	3.8×10^{-3} per (mg/kg day) in mice
picloram	5.7×10^{-4} per (mg/kg day) in rats
glyphosphate	2.4×10^{-4} per (mg/kg day) in mice

In order to use these potencies in animals to predict cancer rates in humans, we multiply by an interspecies factor K . Then the additional probability of occurrence $P(d)$ of cancer in humans, due to the chemical dose becomes

$$P(d) = K \times b \times d$$

where K is the interspecies conversion factor

b is the carcinogenic potency in animals derived (conservatively) in section 3,

d is the average daily dose over a lifetime measured as a fraction of body weight (mg/kg day).

In turn, $d = D \times N/L$ where D is the daily dose

N is the number of days during which the dose D occurs during an individual's lifetime

L is the number of days in a lifetime, taken to be 25,550 days in a 70 year life span.

This assumption that the average lifetime dose in animals may be compared to a few individual doses in man by this formula $d = D \times N/L$ is not very well studied experimentally. There are some studies which suggest that single doses of a chemical may be more important than the same total amount of chemical spread out in the many daily doses. This is to be expected if the dose-response relationship is non-linear, but is generally believed that if low dose linearity is assumed, taking the simple average dose by use of this formula is adequate.

The factor K is taken to be given by the assumption that it is surface area not body weight that matters in the interspecies comparison, notwithstanding the data of Crouch and Wilson and Crump, et al. , following a suggestion of Mantel & Schneiderman (1975). This practice is used by EPA. (Anderson, et al. 1983)

Then K becomes

$$K = (\text{human weight/test animal weight})^{1/3}$$

for an average human weight of 70 kg [You had 50kg in the test! You must have been a POW] and an average rat weight of 350g, K is estimated to be ≈ 5.2 .

for an average mouse weight of 50g, K is estimated to be ≈ 15 .

Page 28: Add after Comparison of Cancer risk with other common risks.

It must be emphasized that these risks are "best estimate" risks, and for cancer risks "best estimate" risks in the framework of the linear no-threshold formula. The conservatism of this section were not present and in particular K was chosen equal to 1. Thus the risks in the table are not strictly comparable to those calculated in the rest of the EIS. To make the cancer risks comparable, the risks in the table should be multiplied by about 20, and the amount to accumulate a one in a million risk of death divided by the same factor of 20. Thus 2 diet sodas containing saccharine give a conservatively calculated risk of one in a million annually. Just 1/30 of a diet soda gives a conservatively calculated lifetime risk of one in a million.

Change Table 5-15; After:

"6 pounds of peanut butter
(aflatoxin B1)
1800 pints of milk (c)
(aflatoxin G)

c) modified from the reference to take account of the less toxic aflatoxin that is present in milk

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ADD at middle p. 28

Comparison of this to other approaches

Many authors, Peto (1983) and Ames, et al. (1987) believe that the twofold uncertainties in interspecies comparison and high/low dose extrapolation are so great that describing the cancer risk quantitatively in this way is misleading. Ames, et al., for example, prefers to stop short at a statement of a ratio of the daily dose of concern in humans (expressed as a fraction of body weight in units mg/kg day) to the dose (that they call 7D50) that causes 50% of tumors in animal tests. This ratio they call (human exposure/rodent potency) or HERP for short and they list a number of values for HERP for typical activities (which is reproduced here in Appendix H). [IT IS NOT IN MY COPY!]

It is possible to make a comparison of the risk estimates of tables 5-11, 5-12, 5-13, 5-14 and C161 to C166, with this approach.

By using the simple dose-response formula of section 3, and neglecting background tumors ($a = 0$) the formula becomes approximately

$$P(d) = 1 - \exp(-bd)$$

$P(d) = 1/2$ at the dose where $\exp(-bd)$ is $1/2$. This is described by Ames et al as TD50, so that $TD50 = \ln 2/b$.

The lifetime risk calculated in this section is

$$bd = d/TD50 \ln 2 = (\ln 2)(HERP)$$

Ames, et al., in calculating HERP do not use all the conservatisms mentioned above, and in particular implicitly put $K=1$. Thus the values in the tables 5-11 to 5-14 and C161 - C166 become HERP values when divided by $1/K \ln 2$ or about 3.

In this report, a LOEL-HERP has been used. Again, this can be related to the risk values calculated here if the same data sets are used. A LOES or NOEL for carcinogenics corresponds to a dose where 5% or less of animals get tumors. Taking the value of 5%, we see that at this dose

$$bd \approx 0.05$$

$$LOEL \approx 0.05/b$$

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Then the LOEL-HERP is

$$d \times b / 0.05 \approx 20 d \times b$$

Again the LOEL-HERP ignores the conservatisms of this section and in particular puts $K=1$. So that to go from the tables of 5-11 to 5-14 and C161 to C166 of this section to a LOEL-HERP one must multiply by $20/K$ or about 4.

Table 5-12 last column

Erase the $1/2$ and $3/4$ just before and after the entries.

I now go to the beginning of section 5 "Human Health Risk Analysis" where I find that there is not a proper contrast made between the approach for acute effects (Margin of Safety approach [which I like very much!]) and for chronic (cancer) effects where a risk is estimated. I suggest additions and changes to pages 1 & 2 to accomplish this.

After 1st paragraph of page 1.

"Two distinctly different methods for describing the risks of threshold effects, and non threshold effects. In the first of these, a level is found that is related to the threshold in animals; and the relationship of the dose in humans to the threshold in animals is discussed rather than the risk itself. If the dose in humans is well below the threshold, the risk is zero, if above it is close to unity, and it varies fast in between.

In contrast, for a non-threshold response, there is a finite risk at any level of dose. It is the magnitude of this risk itself which becomes the subject of discussion."

I find the top paragraph of section 5 page 2 incomplete. I would pull out the last sentence and make a new paragraph.

"It should be pointed out that a margin-of-safety greater than unity does not always mean that the dose is safe. A MOS of 3, for example, could represent a high risk of toxic effects for repeated exposures. This occurs because the NOEL is measured only for a single exposure.

-13-

Although we made in the earlier paragraph a sharp distinction between acute effects (occurring probably from a single exposure) and chronic effects (occurring after repeated exposures) the NOEL is measured only for a single exposure. For those exposed repeatedly therefore, a larger margin of safety is appropriate.

Also, for a particular chemical and a particular individual an MOS of 3 could still represent a risk for a single exposure. This can occur if either the individual is more sensitive than the average, or if for this chemical, the human NOEL (if it were possible to measure it) lies lower than the animal NOEL. Conversely and MOS of 1/3 for a particular chemical and individual might be safe. This can occur if the individual is less sensitive than the average, or if for this chemical the human NOEL lies higher than the animal NOEL.

Therefore even if an individual were found who was exposed to a chemical with an MOS of 1/3 without harmful results, this would not invalidate this cautious approach.

Page 3: I would pull out a subheading in the middle of the page (after the first 2 paragraphs):

CANCER RISK

This is because the 3rd paragraph is using the very different procedure and this needs highlighting.

Although I was not asked to comment thereon, I would like to see the excellent summary of human epidemiology used in the same context as the cancer risk estimates from animal data. This would enable us to see if there are inconsistencies.

It is an axiom that the proper study of man is man and that epidemiological data should be used in preference to animal data when available. The detail of the animal data presented here suggests that animal data is superior. However it is important to be sure that it is not contradicted by human data.

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In appendix H, pp. 126-135, the heading is give "Point Estimate of Risk." Yet what is listed is the "Risk ratio." The heading should be replaced by "Point Estimate of Risk Ratio" or confusion will result since this is not the way the word 'Risk' is used elsewhere in the report. (This is stated on page 125). Also on page 125, can Risk Ratio be defined?

Small Items

Reference

Ozkayak & Spengler has a generally available reference. I refer to it in my reply to a letter in Science, September 1987.

Comments on Chapter 4 of Main Report

Page IV-87

The discussion of risk assessment policy (a term used only by this particular NRC committee and nowhere else) is incomplete. I would add at the end of the 2nd full paragraph (after line 15 from the top of the page):

"Because these judgements have such an impact, it is usually considered important for a summary of these judgements to be presented to whoever is making decisions about risk (the risk manager) along with the values of the risk (the risk characterization)."

Page IV-88

The section on Dose-Response assessment mixes and confuses two separate areas of scientific uncertainty. The use of animal data to predict hazard in man, and the use of data at high doses to predict consequences of low doses. I would rewrite the first few paragraphs to clarify this. In particular the paragraph at the bottom of page IV-86 is nonsense as written.

Draft rewrite of Dose-Response assessment section:

"As with hazard assessment there are many scientific judgements involved

-15-

in what we here call dose-response assessment. There are two particular and distinct areas of scientific uncertainty and therefore controversy. The first is how to use data in one species to predict health hazards in another, and the second is how to use data on health hazards, or the probability of health hazards, at low doses. These two areas can be different for each of the toxic hazards considered in the previous section."

2nd paragraph as it stands but last sentence could be expanded: "In general, tests in animals do seem to be predictive, particularly for acute toxicity (Hart & Fischbein, 1985; Davidson, Parker and Beliles, 1986; and Calabrese, 1983). For carcinogenicity, this is also borne out by a comparison of these cases where there exist both animal data and human epidemiology (Crouch and Wilson 1979; Crump, et al. 1987)."

IV-86.

Dose response assessment. Third Paragraph

The mixing here of animal bioassays and human epidemiology is confusing as written. I suggest a small rewrite.

Human epidemiology is often based on exposures for medicinal purposes or in occupational settings in other times where exposures were higher than those we now anticipate to occur routinely.

Animal tests are restricted to tests with a few hundreds of animals, whereas we want to be able to predict small effects in populations of millions. Thus these tests, also, are conducted with exposures (and therefore doses) much higher than expected to occur routinely.

The extrapolation from high doses to low doses demands a knowledge of the shape of the dose-response relationship. This is rarely known well experimentally. Two contrasting shapes are usually used. For acute toxic effects a sharp threshold is usually assumed, whereas for cancer and mutagenesis, a much shallower relationship is assumed usually linear. For the threshold effects of general systemic toxicity, reproductive toxicity, and developmental toxicity, animal "No Observable Effect Levels" (NOEL's) are used

-16-

as an indication of where a threshold might lie. For carcinogenicity some scientists use "lowest observable effect levels" (LOEL's). Others use the data directly, to find the parameters of a theoretically derived or assumed formula relating risk to dose. No dose response assessment is made for neurotoxic and immunotoxic effects.

The dose-response relationship for carcinogens is generally assumed to be linear at low doses (Zeise, et al. 1987). Using such a relationship and some conservative (pessimistic) assumptions, I estimated human cancer risk from animal studies. These estimates are listed in Appendix D.

page IV-92

The first paragraph of risk characterization needs expanding and the second needs modification. I give suggested changes below:

"Risk characterization uses the information gathered in the other stages of the process to represent the overall risk. The assessment of a particular toxic hazard is joined with the level of exposure (by means of the dose-response relationship) to provide a probability of that hazard. Where possible, of course, data on humans are used. However, where there is no such data, or they give upper limits on risk too high to be useful, data on laboratory animals must be used with appropriate conversion factors and uncertainties.

Included in the results of the qualitative analysis are estimates of the NOEL's and LOEL's measured in animals. These measures are presented with an evaluation of the quality and adequacy of the data upon which they are based. Since tests are usually done with about 100 animals, and if less 5% show the toxic hazard under consideration, it is often indistinguishable from background, a NOEL or a LOES do not represent "absolutely safe" levels, but levels at which the risk is 5% or less.

page IV-93, first full paragraph

I would omit "significant" at the end. It waters the whole down so much that there is no meaning. If there really is a threshold, the risk is zero

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for any dose below it. Even for non-threshold phenomena, one can talk about a dose level below which the effect (and risk) is not significant.

page IV-93, last paragraph

Emphasize that is is a lifetime risk. The last sentence becomes

"Thus some level of risk is usually chosen below which the risk is considered "acceptable" or "negligible." This level is frequently chosen to be one additional cancer in the lifetimes of either one hundred thousand, or of one million, people exposed. However, it has often been noted that people are exposed to cancer risks that are 10 or 100 times larger than this without much concern (Crouch & Wilson, 1981; Pochin, 1975)."

Table IV-18

This is confusing. I suggest a changed heading.

"Degree of confidence that risk is small at a given exposure level"

"Ratio of NOEL
to exposure level"

"Degree of
confidence"

Top page IV-94

This end of the paragraph carried over from page IV-93 is wrong as written. It does not require mathematical modelling. Replace with:

"The risk is calculated using a dose-response relationship whose shape cannot be determined directly and must be based on assumptions or theories of carcinogenic mechanisms. It is common, and this approach is adopted here, to assume that this relationship is linear at low doses, and to determine the scale from data on laboratory animals."

Page IV-94

The discussion of Ames procedure is not quite right and too brief. I suggest the following:

"However, some authors, Peto (1983) and Ames, Magan & Gold (1987) believe that the uncertainties involved with such modelling are so great that to present the risk in this form is misleading. Ames, *et al.* (1987) propose an ordered list to compare hazards. They define a tumor dose (TD50) at which 50% of the animals in a laboratory test develop cancer. They then define a Human exposure/Rodent potency or HERP as the ratio of the human exposure to this TD50. This they normally express as a percentage. Ames, *et al.* calculated a number of HERP's for daily or common exposures to put these into context. A list of these HERP's is in Appendix H. [NOT IN MY COPY!]

In this report we also use an adaptation of Ames' HERP. Instead of the TD50 calculated by Ames, we use the LOEL. This then we call the LOEL-HERP. Neither Ames' HERP nor the LOEL HERP are intended to be used as estimates of actual risk to the exposed human population. However they are intended to compare the risks between hazards, and in particular can be used for comparisons among the potential hazards associated with Forest Service use of the 13 herbicides for which there are carcinogenicity bioassay data.

Since the TD50, the LOEL, and the carcinogenic potency used by LAI in their risk assessment are all (in principle) derived from the same data on laboratory animals, it should be expected that the ordering of the potential risks should be the same for the HERP, the LOEL-HERP and risk values calculated in appendix section 5 and their comparison to commonplace risks (Crouch & Wilson 1981). In the appendix section 5 there is a discussion of the relationship between these. In particular, since a LOEL is usually about 10% of a TD50, the LOEL HERP's are expected to be about 10 times Ames' HERP.

Page IV-95, middle of page

Although individual epidemiological studies may not show number of "excess cancers" that are statistically significant, studies can be continued provided that care is taken to ensure that there is no bias; such as selecting only positive studies to combine. You may not have time to make such a compilation, but you should refer to the possibility.

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Page IV-100

Summary

Where are the risk numbers, or upper limits thereof, derived from the epidemiology? "... the overall suggestion that phenoxy herbicides in some way initiate or promote cancer, and that this occurs at a level of exposure experienced in various work settings." How weak for this EIS! How do these levels of exposure compare with levels expected in any of the scenarios for future use of pesticides? Are they higher? Lower? The same? Surely the author of this EIS knows the answer to these questions better than the reader. Unless some indication is given of the answer, this whole section becomes either useless or subject to arbitrary interpretation by the reader.

Page IV-119, 4th Paragraph

The first sentence would read better if expanded

"Public involvement with understanding, and controlling the risk associated with vegetation management..."

Page IV-122, 4th Full Paragraph

B α P is inverted to become P α B.

REVIEW OF HEALTH IMPACTS SECTIONS OF DRAFT ENVIRONMENTAL
IMPACT STATEMENT

A. General comments.

The over-all appearance of the document is impressive. Clearly much effort was put into this project and the organization of this material was a major task. While some of the writing is simple enough to be understood by lay-persons my overall opinion is that it is very difficult to follow the logic, organization and facts. In addition, there are some glaring misintepretations of scientific data and techniques.

I have never read a document of this type previously and may be somewhat naive in my expectatations. However, I have a reasonable knowledge of medical and environmental toxicology and should not have experienced so much difficulty in reading this material.

I am totally neutral with respect to any implied or underlying political messages buried with-in the context of this document. Since this is not an area of my expertise, I probably have missed some of the subtilty. My impression is that the purpose of this document is to propose a course of action for each contingency or anticipated problem. The difficulty with this approach is that real life situations are never the same and common sense must prevail. Ideally, the document should make this point of view understood so that it is not used by bureaucrats to justify an improper action or an inaction, rather than a careful analysis of the problem by experts. In time the data will improve, the experts will gain more experience and the problems will be better defined. Thus, more logical responses will become possible without the constraints of prestated policy and outdated dogma.

Some specific examples of problems with the text:

Appendix D, section 3;

page 2-last paragraph- toxicity is a progression of signs and symptoms which increase with time and dose, from subthreshold effects to lethality. Death is the easiest response to determine since it is an all-or-none response whereas lesser toxicities are not.

page 3-para 3 and 4- ppm in the animal diet is the concentration but not the dose intake into the animal. Must state the daily dose intake per animal and base dosage on this plus the concentration in the food.

The last paragraph should read as follows:
Dosing in longer term studies is generally done by adding to the daily diet specified amounts of chemical in a dose range of ppm (parts per million) in the food. The known average weight of the test animal during the test period, the concentration in the diet and the amount of food consumed daily are used to calculate the daily chemical intake in terms of a daily dosage, i.e., in milligrams of chemical per kilogram of body weight per day (mg/kg/day). This value may be used to extrapolate to a comparable dosage in humans. Generally, at least 3 groups, each receiving a different dosage level, in addition to a control group receiving a zero dose (placebo or diet only) are used for these studies. Animals are initially separated according to sex and then divided into groups containing 8 to 50 animals, according to their assigned dosage schedules.

page 4, 1st para, line 4- the greater the toxic potency not toxicity (which implies efficacy).

line 7- makes no sense. No one sets these dose levels. They reflect the range at which the LD50 effects are observed. They may be lower in chronic studies but that is because the dosage is chosen which will not cause acute death, therefore, the dosage must be lower in a chronic study.

line 7-should read; Because lethality is the endpoint of an LD50 study, the dose range studied in an acute study is usually higher than in a chronic LD50 study.

Table 3-1 the last column should be presented in both metric and English equivalents and based on a 70 kg human.

Figure 3-1, The upper graph is inaccurate and misleading. This is a dose response curve, therefore, the Y axis cannot be used for both number of animals that died (LD50) and a progression of toxic symptoms. Some toxic, moderate toxic effects, etc., are a continuum of increasingly more toxic symptoms which are dose related. Split this graph into 2 separate graphs and then demonstrate the LD50 in one graph and the progression of toxic symptoms in a second graph, both relating to increasing dosage.

The lower diagram is very abstract, inaccurate and misleading. I suggest that it be deleted and the NOEL dose range be demonstrated by shading on

both of the above graphs. The threshold level can likewise be emphasized on these graphs. Thus, the 2 graphs placed on the same page will demonstrate the NOEL, NOEL threshold level, a dose response for the progression of toxic symptoms, and the number of animals dying (separate graph) with a relative comparison of doses for all of the above toxicities.

page 6, 2nd sent. ...dosed during the various stages of gestation. The middle period of gestation is the most commonly examined stage since this is when fetal organs are often most sensitive to chemicals.

3rd sent.....structural deformities after birth.

see xero of page for further suggestions (enclosed).

page 7a Table 3-2 see xerox for suggested changes (enclosed).

Recommendations:

The document requires a reorganization to follow a more logical structure.

1. The pages should be numbered in sequence with a single, well organized index for the entire text,

2. The content should be reviewed to reduce the over-all wordiness, obliterate the jargon and put the concepts in a more simplified and scientifically accurate format.

3. The glossary should be at the end of the document and contain the scientific terms mentioned in the text.

4. Many of the figures are confusing and scientifically inaccurate; tending to combine too many variables in a single figure.

5. A small group of experienced senior experts should meet to reorganize the document.

6. Make a concise statement of purpose, approach and organization of the document.

7. The entire text should be divided and reviewed by an assigned individual in conformity with a preset standard and purpose.

8. Each section should be evaluated, deleted or rewritten with more scientific accuracy and be made more concise.

9. One or two individuals should be responsible for overseeing these revisions, write the necessary bridging comments and collate the sections into a complete document.

10. The final document should be reviewed by all of the participants, as well as other experts and then submitted as a completed document.

I believe that the intent of this document is a worthy task and would serve as a useful guideline, provided that the revision is carefully completed.

Date: February 12, 1988

From: Shelia Hoar Zahm, Sc.D. *Shelia Zahm*

Subject: Review of Draft Environmental Impact Statement,
Managing Competing and Unwanted Vegetation,
October 1987

To: Gary L. Larsen
Group Leader, Vegetation Management

Thank you for this opportunity to comment on the draft environmental impact statement (October 1987) for the National Forests of the Pacific Northwest Region. My comments reflect my personal views as a scientist and do not necessarily reflect those of the National Cancer Institute. I have been asked to comment on the approach which was used to arrive at conclusions on human health risk for use of herbicides in vegetation management.

1. Accepting as given the evaluation and interpretation of toxicologic and epidemiologic studies presented in the Appendices, are human health risks well characterized in Chapters II and IV of the draft EIS?

The review of toxicologic and epidemiologic studies presented in the EIS is comprehensive and up-to-date. To my knowledge, only two recently published cancer epidemiologic studies have been omitted from the EIS. These two studies, Vineis et al. 1987 and Woods et al. 1987, do not change the

conclusions of the EIS in any way. Although my knowledge of toxicologic research is less complete, it also appears that all relevant toxicologic research has been included.

Each study has been interpreted in a conservative fashion, that is, to maximize the estimate of possible risk. For example, the herbicide 2,4-D was assumed to be carcinogenic based on the Hardell et al. and the Hoar et al. epidemiologic studies, although no general consensus to that effect exists in the scientific community.

Based on the comprehensive review and conservative interpretation of the toxicologic and epidemiologic research, human health risks are characterized in Chapters II and IV. All population groups likely to incur risk from herbicides or prescribed burning have been addressed in the EIS, including forestry workers (such as aerial applicators, backpack sprayers, and workers involved with mechanical methods of vegetation management) and members of the general public who might be exposed via direct contact with herbicides or smoke or via herbicide contamination of water or food from forest areas. The appendices contain extensive information on exposure estimates. Routine-realistic and routine-worst case and accidental spraying and spill scenarios have been considered. Potential for synergism has also been addressed. Exposure data have been compared to LD50s, systemic NOELs, and reproductive NOELs. Margins of safety have been calculated.

Human health risks for 16 herbicides have been painstakingly characterized with no obvious sources of risk overlooked. The risk estimates are, of course, limited by the toxicologic and epidemiologic data available. Many of the herbicides have not been adequately tested to allow definitive risk evaluations.

2. Given how human health risks are described in Chapters II and IV, does the draft EIS draw clear and appropriate conclusions from this characterization of risks?

Pages IV: 100-125 present clear summaries of human health risk from smoke and the 16 herbicides under consideration. The conclusions appear appropriate, i.e. conservative, and take into consideration the lack of adequate data for many outcomes. When data are unavailable, safety of the herbicides has not been assumed.

3. Given the characterization of health risks and the conclusions drawn from this characterization of risks in Chapters II and IV, is the analysis of health risks adequately and appropriately included in the evaluation of alternatives?

The analysis of health risks has been incorporated into the evaluation of alternatives with some notable exceptions. Before describing these exceptions, I will name the

alternative that I think best minimizes human health risks and maximizes effectiveness of control and explain the rationale for my choice. The proposed plan has a serious flaw, however, which appears to fail to consider a possible human health risk, and needs revision.

In general, my assessment of toxicologic and epidemiologic research presented in the EIS and in the scientific literature as a whole is that while some pesticides are definitely carcinogenic or otherwise hazardous, I do not believe that all pesticides are harmful to humans. I do not believe it is necessary to restrict use of all pesticides to protect human health. Given the obvious benefits of prudent pesticide use, I believe that Alternative E is a reasonable approach to forestry management.

It is extremely important to limit inadvertent exposures of the general public and dermal exposures of pesticide applicators. Alternative E prohibits aerial spraying, which limits exposures to the general public or its food or water supplies and reduces the public's perception of risk associated with pesticide use in forestry. Burning of herbicide-treated vegetation is also prohibited. Alternative E stresses that safety precautions be taken to reduce risk to forestry workers, which is extremely important since only backpack and ground applications of herbicides will be used. Backpack spraying, in particular, can result in heavy dermal exposure for the applicators. Use of protective equipment

and hygienic work practices must be required to minimize worker health risk.

Alternative E recognizes that specific herbicides pose unacceptable human health risks and should be prohibited. It does not seek to prohibit use of all herbicides, which the scientific data suggest is unnecessary. In my opinion, Alternative E has not adequately incorporated the draft EIS's analysis of human health risk into its selection of the specific herbicides to be used. My greatest concern is with the apparent plan to use atrazine. Chapter II, page 14, lists the herbicides to be excluded in any application (2,4-D, amitrole, diuron, and fosamine) and in backpack spray applications (bromacil, 2,4-DP, and simazine). I am concluding that the lack of mention of atrazine means it will be considered for use.

Chapter IV, pages 110-116 contain a summary of health impacts by herbicide and state that atrazine is one of the "five herbicides...judged to be of particular concern for toxicity." Atrazine has a high cancer hazard compared to the other 15 herbicides. It has adverse reproductive, developmental, mutagenic, and neurotoxic effects. Toxic effects would not be unexpected for both residents with routine spraying or backpack sprayers, according to the EIS. In many scenarios and adverse outcomes, atrazine falls in the same risk category as 2,4-D, which is specifically prohibited in Alternative E, yet atrazine could be used. Also, atrazine

has one of the longer half-lives of the herbicides under evaluation. Not mentioned in the EIS is the fact that atrazine has been found repeatedly in groundwater in the agricultural areas of the midwest, an additional cause for concern.

The use of dicamba and triclopyr may also be questionable, although there is much less adequate data than for atrazine.

4. What recommendations would you suggest for management of risks related to possible exposures to herbicides in forests, considering both forest workers and the general public?

As mentioned in # 3, I believe Alternative E is both prudent and practical, with a few qualifications. I strongly recommend that atrazine be added to the list of herbicides that will not be used in any application for reasons stated above. I also believe that the Forest Service must be responsive to newly released toxicologic and epidemiologic data as it chooses chemicals for each season. Mechanisms must exist to guarantee that the most current data are considered. The list of prohibited herbicides in Alternative E should not be considered "final."

College of Forestry



Corvallis, Oregon 97331-5704

January 14, 1988

Gary L. Larsen
Vegetation Management Group Leader
USDA, Forest Service
Pacific Northwest Region
P.O. Box 3623
Portland, OR 97208

Dear Gary:

Attached please find detailed comments on the draft environmental impact statement "Managing Competing and Unwanted Vegetation." I wish to summarize those comments in this letter for the record. There are seven issues I wish to address: (1) development of alternatives; (2) regional vs. subregional or Forest analysis; (3) the "reference" base; (4) separation of fact, hypothesis and conjecture; (5) similarity of alternatives; (6) unavoidable impacts; and (7) style and organization.

At your request, I have tried to read the document as a member of the general public and focus on broad issues rather than detail. It is within that context that I offer the following observations.

Development of Alternatives

The seven alternatives have been derived from issues raised by public commentary (I-7). They focus, as a result, on the tools or input variables used to manage forest resources and not the output or end product of that management. In their present form, the alternatives relate very poorly to the way in which vegetation management decisions should be made and implemented in the complex mosaic of Pacific Northwest ecosystems. Given the diversity of sites and species and the diversity of management objectives, it is necessary to match the tools to goals for resource management to a particular landscape. Shouldn't this EIS help people understand that rationale rather than force selection of some blanket vegetation management prescription for all of the Pacific Northwest Region? What tools are necessary to reach specific management objectives on particular sites (broadly defined)? What are the costs and benefits of using or failing to use those tools? As presented, the alternatives take on the appearance of an end, rather than a means to an end.

Gary L. Larsen
January 14, 1988
Page 2

Regional vs. Subregional or Forest Analysis

It is impossible to read this document without concluding that the Pacific Northwest Region is highly diverse ecologically, economically and socially. In spite of this diversity, the EIS attempts to average impacts across the region, with occasional hints that some subregions may be affected differently than others. Table IV-14 demonstrates that subregional differences are profound. A Forest-by-Forest analysis would likely show even greater differences. The argument that such analyses are not possible is fallacious. One of the most important comparisons is an analysis of economic impacts of each alternative. The Forest Service IMPLAN model could be used to provide estimates on a county basis.

If this document is to be meaningful to most people, the scale has to be reduced from regional to at least subregional and, preferably, to the Forest level. This is especially true for the economic and social impacts. On a world or national scale, to be absurd, R-6 vegetation management decisions are meaningless. But not in Grants Pass or Roseburg, Oregon.

The "Reference" Base

One of the most confusing parts of the document is the discussion of the "Reference" base for the analyses and the argument for using Forest Plans not yet finalized and confidential budget information. I don't understand the logic and I suspect the public will conclude it is a "smoke and mirrors" analysis. It is particularly confusing because of the admission that the Forest Plans are likely to be changed depending on the outcome of the vegetation management controversy. It is, therefore, cyclical reasoning. Unless there is a clearer, more rational description, I would suggest that the "reference" be the plans presently in force and budget values everyone can examine. The argument made for using draft Forest Plans and confidential budget information (relative change or comparability) is even more valid for existing plans and budgets, in my view.

Separation of Fact, Hypothesis and Conjecture

Throughout the EIS, I found well-documented, clearly stated information of the impacts of vegetation management side-by-side with statements made as fact without documentation, usually with "may be" or "could be" as the only qualification. Most people won't be able to tell the difference.

There is an excellent statement on page IV-7 of the various levels of precision and accuracy of the information used in the EIS. Given this statement, it is imperative that each description of impact be accompanied by a statement of the quality of data, especially when stating conclusions. Many of the impacts are presented as predictions of what "may" or will happen. Readers need to know how accurately those predictions can be made. A good example appears on page IV-67, the conclusion for a section about prescribed burning. This format, clearly stating what is known and not known, should be the pattern for all such statements, not just one. I recommend you rigorously employ it throughout.

Gary L. Larsen
January 14, 1988
Page 3

Similarity of Alternatives

I was struck by the similarity of the alternatives and impacts they create. Statistically, all seem the same except for C which is greatly different from the rest. Must we conclude from this that there is really no difference between any of the various methods and the only issue is to treat or not treat? Or is this the result of regionwide "averaging"? If so, this argues strongly, again, for subregional analysis.

Unavoidable Impacts

The section on unavoidable adverse impacts describes an important concept that should be introduced in the early part of this EIS rather than buried in the back (IV-134). Throughout the EIS are suggestions that the procedures prescribed by the Manual for mitigation of impacts are sufficient to prevent any adverse change. This section on unavoidable impacts candidly admits that isn't possible. Adhering to the concept in description of likely changes from vegetation management operations will add credibility to the EIS. Moving it to an earlier place in the EIS will help readers better understand the issues.

Style and Organization

After reading the EIS, I tried to put myself in the place of a non-technical reader to determine what would have helped me understand the issues and alternatives better. A more journalistic style would improve the readability of this EIS. By that I mean more attention to placing important facts and key conclusions in lead sentences and paragraphs. There is a tendency to bury the "punch line." Consider the section on public health protection from smoke (IV-40, 41). One reads nine paragraphs of all the hazardous compounds in smoke and that inhalation can cause death. But buried in paragraphs five and nine are sentences that say "very little hazard exists for the public" and "the concentration of toxic compounds downwind of the fire is probably too low to cause measurable health effects." One ought not to have to work that hard to find this out.

Pay careful attention to the wording of conclusions, because people may read only that part. Many concluding statements are poorly worded. Some are badly misleading. Others simply rehash facts. At a minimum, they should have a common format and style that clearly states your best estimate of impact and how certain or uncertain you can be that your estimate is accurate.

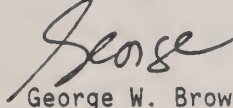
Strongly consider reorganization of Chapter 3 and parts of Chapter 4. Chapter 3 reads like "boilerplate," i.e., something that could be a part of any EIS written for any activity in the Pacific Northwest. The real issue is not what the geology and climate are, but how these and other variables interact to affect the choice of vegetation managements tools and are, in turn, affected by the choices made. I suggest that information be brought together in one place, synthesized and interpreted, not just described.

George W. Brown

Gary L. Larsen
January 14, 1988
Page 4

I hope my comments are useful in your revision of this draft. Please call if I can be of assistance in helping interpret or clarify my remarks.

Sincerely yours,

A handwritten signature in cursive script, appearing to read "George".

George W. Brown
Associate Dean for Research

jlh
Attachment

<u>Page</u>	<u>Detailed Comments</u>
II-29	The comparison of social and economic impacts of the alternatives provides very little help to a reader trying to decide how his/her community will be impacted. For most people, such an analysis made at the regional level has no meaning because of scale. While the values are very high, later discussion in Chapter IV implies that they are really insignificant on a regional basis. The real issue is Table IV-14. The social and economic impacts are not uniformly distributed across Oregon and Washington. Why not use the information in Table IV-14 to provide readers with some indication of how jobs, income and payments to local governments will be impacted by subregion or Forest? If that is done, statements such as "the changes ... are well within the usual variation" and "are ... significantly less disruptive than the economic fluctuations experienced in the region's communities over the last ten years and would be difficult to distinguish from other effects" may have to be revised.
II-32	I don't understand the last sentence. How can volume lost on one Forest be made up by increasing the volume harvested on another? The alternatives presented will apply to all Forests, so that if herbicides are removed as a tool, it will be done regionwide. How will losses be made up if the decision is applied everywhere? And aren't Forest plans developed independently? Is there national policy in place to allow "make-ups"?
II-36	<p>This section needs a carefully written description of risk and some explanation of scale. The phrase "potential impacts" is very vague. People need to understand that all "impacts" or changes are not necessarily adverse and that some adverse impacts are unavoidable. That point is well stated on p. IV-134; it needs to be made earlier.</p> <p>Early in the discussion of the environmental effects of the alternatives, the reader needs to understand how conclusions were developed and with what level of accuracy and precision they can be regarded. Are they accurate and precise, developed from detailed measurements and analysis or are they general estimates composited by questionnaire? Each concluding statement should be accompanied by a statement of how confident the Forest Service is of its accuracy. For example, beginning at the bottom of II-39, one finds the statements "there will not be a significant variation among alternatives in effects on riparian vegetation" and "there will not be a significant variation in effects on fisheries resources" and "There is a low probability that any of the alternatives will result in a significant adverse impact on the viability of any wildlife population in the Pacific Northwest Region." Those are very sweeping statements. Not only are they undocumented, they are unqualified as absolute truth. See the discussion for IV-7.</p>

-2-

- II-41 What does "Comparing the Effectiveness of Techniques Dimension of the Alternatives" mean in English? Paragraph 1 seems to mean "how accurately can we prescribe treatments." Paragraph 2 seems to mean "how effective are the various methods for controlling competing vegetation." Paragraph 3 is a truism. Table II-13 seems to say that all alternatives except C are the same.
- II-42 Each alternative should be more clearly described as to its impact by making the vegetation complex presentations consistent for each and by consistently describing harvest impacts by subregion.
- If Alternative B allows all tools and techniques in the "appropriate environmental setting," why does Alternative D "encourage more creativity"?
- II-68 How many herders are necessary to control animal movement under various scenarios? Are they available? Be specific.
- II-69 Uneven and multiaged management have some significant disadvantages. A clearer explanation of advantages and disadvantages is needed.
- II-70 "Livestock will be strictly controlled in the vicinity of wetlands and riparian areas...." They must be controlled everywhere if this method is to be effective.
- Prescribed burning is described as being environmentally correct and very cost-effective "in the right situation." What are these? The discussion leaps immediately to disadvantages.
- II-73 The mitigation measures suggest that you have a high degree of control over fire. Do you? Can you document your success? Why should a reader believe your promises?
- II-75 Paragraph 2, sentence 3 doesn't make sense.
- II-77 The term "intense disturbance" lacks meaning. The issue is erosion and compaction.
- II-78 Why are municipal watersheds the only place where tractors are prohibited if soils have high erosion and sedimentation hazard?
- III-1-53 This chapter reads like "boilerplate." Rarely is there any interpretation of how geology, soil, vegetation complexes, etc., affect vegetation management decisions or are, in turn, affected by vegetation management. The "interactions" sections provide very little useful information in the context of this EIS. If it must be included, I'd suggest moving it to the front as an introduction. Its current placement interrupts the flow of the text.

IV-4 The discussion of "The Reference" and "How the Effects Were Estimated" is perplexing. Readers are asked to take the projections as an article of faith. The "logic" goes something like this for "The Reference":

- The Region has a set of proposed Forest plans.
- The Forest plans are in draft and may be changed.
- One factor which may change the plans is the decision on this EIS.
- But we intend to use draft plans as a reference.

What is known, at present, is the existing Forest plans. Why aren't they "The Reference"? This seems to me like the equivalent of statistical autocorrelation.

The "logic" for the baseline for data collection and analysis is equally obscure:

- Fiscal 1989 is the baseline.
- The FY 89 budget has not been approved.
- The FY 89 budget figures are confidential.
- We can't show you the figures we use, but trust us, everything is fully comparable.

If comparability is the key issue, why not use a set of budget figures that are known and available to the public?

This section also argues for commonality between Forests for vegetation management activities and outputs. Given the autonomy of Forests in developing their plans, simply achieving commonality in "definitions" is hardly the point. The real issue is whether the models used by the Forests to predict changes in outputs are the same. My limited experience with erosion and water quality models used by the Forests in the planning process suggests they differ.

IV-7 The section on "Uncertain Data and Estimates" is very candid and well taken. What it means for this EIS is that the writers must take special care to let the reader know exactly the basis for conclusions drawn or statements made. Throughout the EIS, conclusions based on extensive research are lumped with conclusions drawn from unknown levels of investigation. Readers need to know how confidently (in a scientific sense) can these statements be made. "A clear basis for choice," in my view, must include a clear understanding of the certainty with which projections of impact can be made. "Reasonably estimated" or "reasonably well understood" won't do.

- IV-13 Why can't these sections be combined with those in Chapter III so that readers can consider them together? For example, why not describe the geology, climate and soil, show how they affect vegetation and vegetation management decisions and then describe how they are affected by vegetation management prescriptions and do it all in one place rather than 50 pages apart?
- IV-15 In the discussion of each vegetation management tool, the predictability of impact should be clearly described. Some impacts, such as soil compaction, are highly predictable because of the availability of soils maps, ability to regulate vehicle traffic and good research information on the way soils are affected by vehicles. Other tools, such as prescribed burning or biological methods are less easily predicted. Compare, for example, the scientific citations noted in the herbicide section with those in the biological section. Also note that there are no citations in the prescribed fire section that deal with predicting fire behavior and linkage of that prediction to predictions of environmental impacts. Predictability is particularly crucial in the section on Potential Cumulative Effects. This section is filled with speculation about what these effects may be. The truth is, we don't have the technology to predict cumulative effects with any accuracy, and this section should clearly say so.
- IV-25 Table IV-9 uses mg/L and ppm interchangeably. Readers without a scientific background will be unnecessarily confused.
- IV-29 Is there any documentation of the way in which wildfire spreads through plantations having received a vegetation management treatment and those without?
- IV-31 On what basis are the rather precise-sounding projections of emissions made? How accurate are these projections?
- IV-38 We are assured that "the best available technology will be used to minimize smoke emissions." What technology and how precisely can it be employed?
- IV-40-41 The section on Public Health Protection needs to be revised. Is there a significant health risk? Answer the questions in the first sentence. The readers has to weave through a lot of speculation and "potential" risk information only to find out at the end of the next-to-last paragraph that "the concentration of toxic compounds downwind of the fire is probably too low to cause measurable health effects." Then we find that "rural dwellers will probably be exposed to more risk" than urban people. But if the previous statement is true, no one is really at risk.
- IV-42 Buffer strips are touted as a mechanism to filter sediment. They don't work that way in steep terrain with highly dissected topography.

- IV-43 The conclusion for prescribed burning, biological methods and herbicides is that mitigation measures are expected to be completely successful in preventing adverse impacts on water. How does this match up with the first sentence on Unavoidable Adverse Effects (IV-134)?
- IV-47 Table IV-13 could be improved by adding a column listing recommended maximum concentrations for each chemical.
- IV-48 The probability that reductions in evapotranspiration and attendant increases in soil moisture will lead to increased mass wasting and erosion is near zero! Changes in timing and quantity of runoff are not necessarily harmful.
- IV-52 The flaw in presenting timber yield impacts for each alternative, again, is the issue of averaging across the entire region. Indeed, variation by subregion is discussed for Alternative A and F (but no other) and then in a separate section (IV-56-57 and Table IV-14). Why not take the next step and talk about impacts by Forest? That is what people can relate to and understand and the technology is available through IMPLAN.
- Again, the section on subregional effects makes a strong case that using the Forest plans as a basis for comparison, with all their inherent uncertainty, is a bad decision. The range in differences (Table IV-14) is huge!
- IV-61 "Effects on Diversity," sentence 1, is absurd. If herbicides are not intended to kill the competing vegetation, what are they intended to do?
- IV-63 The same argument made for disaggregation of effects on timber yield is equally valid for impacts on wildlife.
- The last paragraph is an excellent example of the need for candor about what is known and not known and what can and cannot be accurately predicted. This paragraph implies that we know these changes occur, we've documented them and they can be predicted. The rest of the section follows the same pattern.
- IV-65 The use of vague innuendo continues through discussion of cumulative effects and the conclusion of manual methods. No documentation, no explanation of what can and can't be predicted.
- IV-67 Contrast the above with the first paragraph of the conclusion on prescribed fire. In my opinion, that is the type of candid statement of fact which should serve as a model throughout this EIS.
- IV-69 The conclusion for grazing of domestic livestock isn't a conclusion at all, merely a restatement of fact.

The Chemical Methods section opens with a statement sure to scare any lay reader, unless they read paragraphs 3 and 4, where it's clear the chemicals used pose no real threat.

IV-71 If the chemicals used really aren't much of a threat, why doesn't the conclusion say so? A reader skimming this document and reading only the conclusions would be misled.

IV-74 The same argument can be made regarding the conclusion about riparian resources and fish. Agreed, risk increases with acres treated, but no mention is made of the low risks described in the previous text.

The section on Cumulative and Synergistic Effects is poorly done. My research (Brown, 1974) is not correctly cited, among other things. It is a portrait of disaster, but one not even documented with regard to vegetation management methods. And much of it is clearly misleading. If clear-cutting and burning doesn't significantly damage fish habitat, when done properly, why should vegetation management using manual, chemical or prescribed fire methods? That is alluded to in paragraph 4, but still not clearly stated.

IV-85 I'll leave the health effects to toxicologists.

IV-129 The fact that national economic factors play a more important role than vegetation management practices in the regional economy is irrelevant. So is the last recession. Those factors are beyond your control and everyone is affected. The vegetation management alternative will selectively affect communities in the region and that is within your control.

IV-130 Local changes can be analyzed. IMPLAN can do it on a county-by-county basis. Why wasn't that tool employed?

IV-132 Paragraph 2 seems to sweep all the economic changes under the carpet with the statement that "they are, on the average, significantly less descriptive than the recently experienced changes in the region's communities." Again, the issue isn't "on the average" and that recession was one of the worst in the last four decades. There was great social pain. It is an inappropriate dismissal; the issue demands better analysis.

IV-134 The first sentence says long-term productivity is not understood. Then you proceed to predict how each alternative affects long-term productivity.

The statement about unavoidable adverse impacts is a good one and should be prominently displayed in the front of this EIS.

IV-135 How precise and accurate are the estimates of risk?

Review of Appendix A: Timber Growth and Yield Analysis
U.S. Forest Service
Draft Environmental Impact Statement

Appendix A: Timber Growth and Yield Analysis was independently reviewed by Steven Radosevich, Robert Wagner, and Daniel Opalach at the request of USFS personnel. The following report is an attempt to summarize these reviews and to provide input to the USFS concerning their Draft Environmental Impact Statement. The review consists of both general and specific comments. General comments represent a consensus among the three reviewers of common concerns and suggestions for improvements of the document. Specific comments relate to particular points in the DEIS that are of concern or may be improved. Dr. Radosevich has provided reviews concerning this document on two other occasions to various members of the USFS. Mr. Wagner and Mr. Opalach had not seen the document before this review.

General Comments. Considerable improvement of the DEIS can be achieved by careful detail in the following three areas: (1) assumptions used in developing the growth and yield projections, (2) adequate description of the methodology used in making the projections, and (3) assessments of confidence, reliability, or accuracy of the projections.

Assumptions.

Some assumptions need to be made for a project of this type. However, it is important that the assumptions be clearly defined and that the basis for each assumption is substantiated. Mr. Wagner (see specific comments) points out that some of the assumptions made in this document may be problematic, which casts doubt on the entire premise for the analysis. The problem is that a clear statement of management objectives is not presented in this document. Rather, the means for achieving objectives and the objectives themselves (whatever they are) become confounded and confusing. The assumptions made for this section of the DEIS reflect this apparent lack of direction in the DEIS.

Since assumptions are one of the ways that validity for the projections made in the document is achieved, it is important to

indicate how these assumptions were derived. What is the basis, i.e., logic and information, used or developed in arriving at these assumptions? Greater documentation and substantiation is necessary than is provided in the current document.

Methodology.

Yield Reductions (page A-8). Managed yield tables do not represent the "optimum" yields from lands suitable for intensive management. Tables such as these contain yield estimates that are based on limited data sets. Optimum yields will not be known for many years until data are available. For example, the data used to construct DFSIM did not contain a single plot from a thinned plantation with a total age over 45 years (see attached Figure). Furthermore, there were no data for stands over 80 years (Curtis et al. 1981). Since projections in the DEIS are for stands up to 90 years old, it is misleading to claim that DFSIM can generate "optimum" yields for these stands. The same comments pertain to yield projections made with DFIT, SPS, or Prognosis. [Note: The yield tables used by Turpin et al. (1980) and Knapp et al. (1984) to determine the effects of competing vegetation on yield for the Douglas-fir/alder vegetative complex were derived from DFIT (Bruce et al. 1977)].

Brodie et al. (1987) and Stage and Boyd (1987) discuss methods that have been used to estimate the effects of vegetation management on stand yields. The approach used in the DEIS is similar to what Brodie et al. (1987) call the "simulated managed stand comparison method." The primary difference between the two approaches is that Brodie et al. (1987) project the development of existing stands whereas the Forest Service must project the development of hypothetical stands. In the DEIS, the hypothetical stands (aged 15 to 20) are obtained by projecting the height and diameter of very young stands (aged 0 to 10 years).

The final EIS should reference Brodie et al. (1987) and briefly discuss the "simulated managed stand comparison method." Next, the

EIS should describe how the Forest Service determined yield reductions. It is important to compare the two approaches. We believe that the method discussed by Brodie et al. (1987) is defensible from a "scientific" viewpoint. Thus, it is in the Forest Service's interests to link their approach to that of Brodie et al. (1987).

Other than the papers mentioned above, the literature contains little information on how to estimate the effects of vegetation management on long-term yield.

Stratification and Methodology (page A-9). Analysis Step 2 should be described in more detail. It is not clear how the site index of "representative" data sets entered into the computations. Furthermore, in paragraph 6 on page A-10, it is stated that "modal site qualities for each vegetative complex will be used." Was site index ignored when making projections of height and diameter increment trends? Many important details concerning the techniques used to link short-term growth effects and yield simulators are omitted from the DEIS. For this reason, it is difficult to evaluate/criticize the methodology used.

The DEIS does not say how height and diameter trends were projected. It is our understanding that curves were subjectively determined and, therefore, contain an unknown bias introduced by the analyst. The problem with this procedure is that it is unlikely that another analyst would arrive at the same conclusions.

In the DEIS, height and diameter are projected as linear functions of age. Much of the published literature that contains early height and diameter growth trends indicates that exponential functions may be more appropriate (e.g., Preest 1977, Allan et al. 1978, Howard 1979, Arnott 1986, Ross et al., 1986, and Harrington et al. 1988). Furthermore, using linear extrapolations underestimates the effect of vegetation management. Perhaps log transformations of the dependent

variables (height and diameter) may produce more realistic projections.

If possible, height and diameter projections should be performed with a statistical procedure. If subjective methods are preferred by the Forest Service, then a short explanation describing their reasons for this preference should be included in Appendix A.

Confidence and Reliability.

Growth and yield projections are made in this document that vary among the alternatives by only a few percentage points. The results of these projections imply that the "models" used to make these projections are accurate to a fine level of detail (i.e., 1 percent). All reviewers question this implication and suggest that the "models" be examined through sensitivity analysis. Such an analysis would attempt to determine the impact of height and diameter projections on yield estimates. For example, consider the Douglas-fir/hemlock/salmonberry/herbaceous analysis. Suppose "free-to-grow" curves differed from "no management curves" by an additional 10 percent. What effect would this have on volume loss? What effect would an additional 100% have? This procedure would provide a means for evaluating the variability associated with yield projections. We submit that it is unlikely for these projections to be accurate to within 1 or 2%. Thus, it is necessary to indicate and document how well the "models" perform.

SPECIFIC COMMENTS ON THE DRAFT ENVIRONMENTAL IMPACT STATEMENT (D. Opalach)

PAGE A-3, PARAGRAPH 4

The relationship between volume production (yield) and competition shown in Figure 2 (Attachment No. 9) is not consistent with results reported in the literature. Results contained in Wagner et al. (1987) show that this relationship should be described by a curve of the opposite concavity.

PAGE A-7, PARAGRAPH 2

Characterizing the literature as "erratic" is an unnecessary criticism.

PAGE A-10, PARAGRAPH 1

The yield simulation process records on file should be compactly summarized and included in the final EIS. They contain important details and should be accessible to the interested reader. The tables should clearly display the input variables required by the simulation models and important output variables that are eventually used to compute volume loss. A table should be produced for each vegetation complex.

PAGE A-11, DOUGLAS-FIR/ALDER ANALYSIS

Since the yield estimates are taken from Turpin et al. (1980) the DEIS should have included a discussion on their assumptions and methods. Were they comparable to the methods and assumptions used in the DEIS? In the information review much of the available literature is described, but none of this material is used to determine yield estimates. Why make the reader review this material if it is not going to be used in the analysis?

The results given by Turpin et al. (1980) might be used as a check on results derived independently for the EIS. That is, yield estimates could be determined for this complex in the same fashion as for the other complexes and then compared to Turpin et al. (1980). If these two sets of analyses lead to similar conclusions, then this would provide some support and justification for the analyses contained in the DEIS.

PAGE A-25, DOUGLAS-FIR/PONDEROSA PINE/CEANOTHUS SPP./HERBACEOUS ANALYSIS

The growth model used to estimate the 39% yield reduction should be identified. Also, would it be possible to rerun the analyses for this complex using ORGANON? Since ORGANON is based on growth relationships and

data specific to forest types located in southwest Oregon, the results may be more accurate than those reported in the DEIS.

PAGE A-29, DOUGLAS-FIR/TANOAK/MADRONE ANALYSIS

Would it be possible to rerun the analyses for this complex using ORGANON? The results may be more accurate than those obtained with DFSIM.

PAGE A-37, LTSY EQUATION

The notation used in LTSY equation is poor: the asterisks following the variables A, B, and C are not needed. Although D is not used in the equation, it is defined immediately below the equation. Furthermore, the equation should include a multiplier of 100 to obtain an answer expressed as a percentage.

The computations on pages A-37 and A-38 could be presented better. Two and three digit results are given in the DEIS. Since many of the terms in the equation only have two digits, the result should contain only two digits.

Specific Comments on USDA DEIS on Managing Competing and
Unwanted Vegetation: App. A, Timber Growth and Yield Analysis
(R. G. Wagner)

General Comments

I recognize the very difficult task the EIS team had in preparing this section. The data for young stand growth and yield in relation to inter-specific competition is not readily available, especially in relation to treatment efficacy and long-term projections.

The major weakness of Appendix A is the lack of information about the growth and yield projections for each alternative. This lack of information made it nearly impossible to evaluate the credibility of the conclusions. The main limitations were:

- Loosely defined or unstated assumptions
- No statement of inputs (site index, thinning, fertilization, etc.) in the runs
- Assumptions were often speculative and arbitrary when made
- No estimates of error or confidence--in fact, very misleading when stating 1-2% estimates of error on A-34 to A-42.
- Not enough summary output information from reported runs.

Providing the necessary information outlined above would be my first recommendation for improving Appendix A. These omissions provide the greatest obstacle to critiquing the document.

A major flaw is the basic premise of the analysis. It assumes some intrinsic difference among the tools of vegetation management, and a resulting effect on timber growth and yield. This is a problematic assumption that invalidates the whole premise of the analysis.

Sound vegetation management is conducted by first, defining the stand management objectives (e.g., 95% survival and a growth rate consistent with some standard for that site). Second, based on an assessment of vegetation conditions on a site, and an understanding of how the vegetation will likely limit (in quantitative terms) achieving the objectives, various treatment alternatives (including no treatment) to manipulate the vegetation (to a defined level) are considered.

Any level of vegetation manipulation is possible with one or a combination of the tools presented (chemical, thermal, manual, or

biological). Therefore, the management objectives (which have not been defined) can be accomplished with any of the tools. The EIS alternatives seem to combine, and therefore, confuse the objectives with the means to achieving the objectives.

Finally, a comparison of vegetation management tools, is based on the safety, environmental impact, cost, availability, etc. of all of the tools that can be used to accomplish the stated stand management objectives.

The growth & yield projections assume a mean tree response. Recent evidence from Petersen (1988) suggest that substantial differences in the distribution (or stand table) develop in stands where weeds are controlled or not. Much of the final stand yields and financial value will be linked to differences in the diameter distribution, not a mean tree response. Differences in distributions could not be calculated, however, the DEIS with the current scientific information, should state this limitation to the analysis.

Linear projections of exponential responses (Attachment 1-7) may not be reasonable. Nearly all available long-term data sets indicate diverging exponential trends in tree growth under low and high competition regimes. These linear projections probably underestimate competitive effects on stand development.

Specific Comments on Sub-sections:

Introduction:

PAGE A-2, PARAGRAPH 2. The first sentence states that "even with intensive management, it is unlikely that maximum yield (the full biological site potential) and the elimination of all competition will be achieved..."

This statement presents a rather poor statement about silviculture on USFS lands. The public is told not to expect site potential yields and that the cost of doing business is unlimited.

This introductory statement needs revision with reference to following points:

1. Maximum yield of what? The previous statement implies management for multiple resources. Is this maximum yield of all forest resources or just timber? The public might accept less than potential timber as a compromise to also producing something else. But I would think the

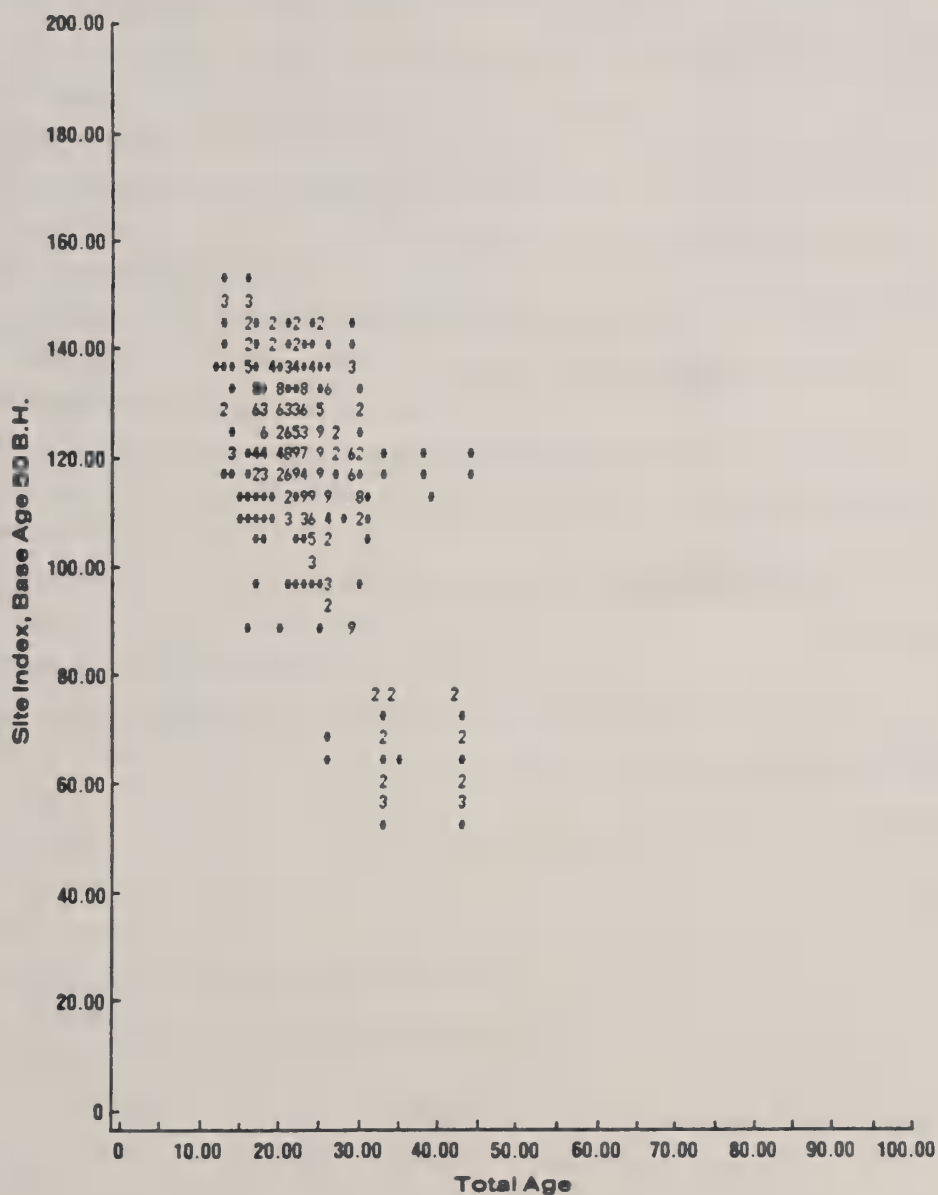


Figure 1. Distribution of data from thinned Douglas-fir plantations used to develop DFSIM. (Source: Figure 5 from Curtis et al. (1980).)

USFS would want to manage for at least a maximum biological potential of multiple resources, especially if funds are unlimited.

2. The time frame also needs to be defined. It is possible to have complete elimination of competition on some sites and a resulting maximum biological potential of plantation growth for a short time. This is currently done after prescribed burning or on grassy sites with good weed control. Complete vegetation control might be a reasonable silvicultural objective for the first year or so of plantation establishment, even though it may be environmentally and economically infeasible for extended periods during the rotation.
3. Not achieving site-potential yields follows further in the analysis when untreated control plots are set as the standard for comparison, rather than achieving normal yield table standards or at least survival standards. The EIS shows little evidence of what the forest management objectives are.

A-3, PARAGRAPH 4. The DEIS states that a reduction in cover will result in response from the crop tree. This statement is not true according to existing data. The negative hyperbolic curve describing the relation between yield and competition level indicates a substantial range of competition where little or no difference in yield results.

Assumptions:

A-4, PARAGRAPH 2 How can broadcast burning be considered available in the assumptions?

1. Alternative F presumes no burning
2. Prescribed burning is a vegetation management technique, yet is included in the vegetation management yield projections.

A-4, PARAGRAPH 3 Substitutability of vegetation management tools is assumed in the growth and yield analysis.

1. This assumption appears to be a contradiction since projecting yield differences among the tools is the basis of the analysis.
2. While this assumption may be fine in an academic framework, it is not realistic (as the DEIS points out) in practice. Therefore, when projecting yields based on methods, non-substitutability should be evaluated as part of the growth & yield projections among alternatives.

A-4, PARAGRAPH 5 "Vegetation control is geared toward conifer establishment (survival) and growth." This statement should include some mention of the growth and survival threshold concept which indicates that conifer growth and survival are not necessarily achieved simultaneously (Wagner et al. 1987).

A-5, PARAGRAPH 8. The DEIS does not describe the quantitative relation assumed between conifer seedlings and the volume of associated vegetation

A-6, PARAGRAPH 8 This paragraph describes an opposition to complete vegetation removal. But the USFS does this in practice all the time with prescribed burning. The DEIS should elaborate on the time frame involved. What are the positive and negative trade-offs the DEIS is trying to balance?

A-6, PARAGRAPH 3 A "degree of caution and conservatism is incorporated into the analysis." How? What?

A-6, PARAGRAPH 5. Implies that trees "grow through competition." Trees may grow above surrounding vegetation, but there is little evidence that they escape the competitive influence.

A-7, PARAGRAPH 1. Define "free-to-grow." This is an undefined silvicultural standard.

A-7 Literature is "erratic and conflicting"? If EIS team would review the literature presented in Stewart et al. (1984), a consistent trend in the biological principles would be revealed. Stand volume growth increases of 40 to 100 percent are common throughout the forest vegetation management literature. This statement should be elaborated or eliminated.

A-8, PARAGRAPH 1. "large statistical error terms...are adequate to display magnitude of change among alternatives."

Without confidence limits on projections, it is unreasonable to assume that reasonable comparisons of the alternatives can be made.

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Review of Appendix A: Timber Growth and Yield Analysis

U.S. Forest Service

Draft Environmental Impact Statement

Appendix A: Timber Growth and Yield Analysis was independently reviewed by Steven Radosevich, Robert Wagner, and Daniel Opalach at the request of USFS personnel. The following report is an attempt to summarize these reviews and to provide input to the USFS concerning their Draft Environmental Impact Statement. The review consists of both general and specific comments. General comments represent a consensus among the three reviewers of common concerns and suggestions for improvements of the document. Specific comments relate to particular points in the DEIS that are of concern or may be improved. Dr. Radosevich has provided reviews concerning this document on two other occasions to various members of the USFS. Mr. Wagner and Mr. Opalach had not seen the document before this review.

General Comments. Considerable improvement of the DEIS can be achieved by careful detail in the following three areas: (1) assumptions used in developing the growth and yield projections, (2) adequate description of the methodology used in making the projections, and (3) assessments of confidence, reliability, or accuracy of the projections.

Assumptions.

Some assumptions need to be made for a project of this type. However, it is important that the assumptions be clearly defined and that the basis for each assumption is substantiated. Mr. Wagner (see specific comments) points out that some of the assumptions made in this document may be problematic, which casts doubt on the entire premise for the analysis. The problem is that a clear statement of management objectives is not presented in this document. Rather, the means for achieving objectives and the objectives themselves (whatever they are) become confounded and confusing. The assumptions made for this section of the DEIS reflect this apparent lack of direction in the DEIS.

Since assumptions are one of the ways that validity for the projections made in the document is achieved, it is important to

indicate how these assumptions were derived. What is the basis, i.e., logic and information, used or developed in arriving at these assumptions? Greater documentation and substantiation is necessary than is provided in the current document.

Methodology.

Yield Reductions (page A-8). Managed yield tables do not represent the "optimum" yields from lands suitable for intensive management. Tables such as these contain yield estimates that are based on limited data sets. Optimum yields will not be known for many years until data are available. For example, the data used to construct DFSIM did not contain a single plot from a thinned plantation with a total age over 45 years (see attached Figure). Furthermore, there were no data for stands over 80 years (Curtis et al. 1981). Since projections in the DEIS are for stands up to 90 years old, it is misleading to claim that DFSIM can generate "optimum" yields for these stands. The same comments pertain to yield projections made with DFIT, SPS, or Prognosis. [Note: The yield tables used by Turpin et al. (1980) and Knapp et al. (1984) to determine the effects of competing vegetation on yield for the Douglas-fir/alder vegetative complex were derived from DFIT (Bruce et al. 1977)].

Brodie et al. (1987) and Stage and Boyd (1987) discuss methods that have been used to estimate the effects of vegetation management on stand yields. The approach used in the DEIS is similar to what Brodie et al. (1987) call the "simulated managed stand comparison method." The primary difference between the two approaches is that Brodie et al. (1987) project the development of existing stands whereas the Forest Service must project the development of hypothetical stands. In the DEIS, the hypothetical stands (aged 15 to 20) are obtained by projecting the height and diameter of very young stands (aged 0 to 10 years).

The final EIS should reference Brodie et al. (1987) and briefly discuss the "simulated managed stand comparison method." Next, the

EIS should describe how the Forest Service determined yield reductions. It is important to compare the two approaches. We believe that the method discussed by Brodie et al. (1987) is defensible from a "scientific" viewpoint. Thus, it is in the Forest Service's interests to link their approach to that of Brodie et al. (1987).

Other than the papers mentioned above, the literature contains little information on how to estimate the effects of vegetation management on long-term yield.

Stratification and Methodology (page A-9). Analysis Step 2 should be described in more detail. It is not clear how the site index of "representative" data sets entered into the computations. Furthermore, in paragraph 6 on page A-10, it is stated that "modal site qualities for each vegetative complex will be used." Was site index ignored when making projections of height and diameter increment trends? Many important details concerning the techniques used to link short-term growth effects and yield simulators are omitted from the DEIS. For this reason, it is difficult to evaluate/criticize the methodology used.

The DEIS does not say how height and diameter trends were projected. It is our understanding that curves were subjectively determined and, therefore, contain an unknown bias introduced by the analyst. The problem with this procedure is that it is unlikely that another analyst would arrive at the same conclusions.

In the DEIS, height and diameter are projected as linear functions of age. Much of the published literature that contains early height and diameter growth trends indicates that exponential functions may be more appropriate (e.g., Preest 1977, Allan et al. 1978, Howard 1979, Arnott 1986, Ross et al., 1986, and Harrington et al. 1988). Furthermore, using linear extrapolations underestimates the effect of vegetation management. Perhaps log transformations of the dependent

variables (height and diameter) may produce more realistic projections.

If possible, height and diameter projections should be performed with a statistical procedure. If subjective methods are preferred by the Forest Service, then a short explanation describing their reasons for this preference should be included in Appendix A.

Confidence and Reliability.

Growth and yield projections are made in this document that vary among the alternatives by only a few percentage points. The results of these projections imply that the "models" used to make these projections are accurate to a fine level of detail (i.e., 1 percent). All reviewers question this implication and suggest that the "models" be examined through sensitivity analysis. Such an analysis would attempt to determine the impact of height and diameter projections on yield estimates. For example, consider the Douglas-fir/hemlock/salmonberry/herbaceous analysis. Suppose "free-to-grow" curves differed from "no management curves" by an additional 10 percent. What effect would this have on volume loss? What effect would an additional 100% have? This procedure would provide a means for evaluating the variability associated with yield projections. We submit that it is unlikely for these projections to be accurate to within 1 or 2%. Thus, it is necessary to indicate and document how well the "models" perform.

SPECIFIC COMMENTS ON THE DRAFT ENVIRONMENTAL IMPACT STATEMENT (D. Opalach)

PAGE A-3, PARAGRAPH 4

The relationship between volume production (yield) and competition shown in Figure 2 (Attachment No. 9) is not consistent with results reported in the literature. Results contained in Wagner et al. (1987) show that this relationship should be described by a curve of the opposite concavity.

PAGE A-7, PARAGRAPH 2

Characterizing the literature as "erratic" is an unnecessary criticism.

PAGE A-10, PARAGRAPH 1

The yield simulation process records on file should be compactly summarized and included in the final EIS. They contain important details and should be accessible to the interested reader. The tables should clearly display the input variables required by the simulation models and important output variables that are eventually used to compute volume loss. A table should be produced for each vegetation complex.

PAGE A-11, DOUGLAS-FIR/ALDER ANALYSIS

Since the yield estimates are taken from Turpin et al. (1980) the DEIS should have included a discussion on their assumptions and methods. Were they comparable to the methods and assumptions used in the DEIS? In the information review much of the available literature is described, but none of this material is used to determine yield estimates. Why make the reader review this material if it is not going to be used in the analysis?

The results given by Turpin et al. (1980) might be used as a check on results derived independently for the EIS. That is, yield estimates could be determined for this complex in the same fashion as for the other complexes and then compared to Turpin et al. (1980). If these two sets of analyses lead to similar conclusions, then this would provide some support and justification for the analyses contained in the DEIS.

PAGE A-25, DOUGLAS-FIR/PONDEROSA PINE/CEANOTHUS SPP./HERBACEOUS ANALYSIS

The growth model used to estimate the 39% yield reduction should be identified. Also, would it be possible to rerun the analyses for this complex using ORGANON? Since ORGANON is based on growth relationships and

data specific to forest types located in southwest Oregon, the results may be more accurate than those reported in the DEIS.

PAGE A-29, DOUGLAS-FIR/TANOAK/MADRONE ANALYSIS

Would it be possible to rerun the analyses for this complex using ORGANON? The results may be more accurate than those obtained with DFSIM.

PAGE A-37, LTSY EQUATION

The notation used in LTSY equation is poor: the asterisks following the variables A, B, and C are not needed. Although D is not used in the equation, it is defined immediately below the equation. Furthermore, the equation should include a multiplier of 100 to obtain an answer expressed as a percentage.

The computations on pages A-37 and A-38 could be presented better. Two and three digit results are given in the DEIS. Since many of the terms in the equation only have two digits, the result should contain only two digits.

Specific Comments on USDA DEIS on Managing Competing and
Unwanted Vegetation: App. A, Timber Growth and Yield Analysis
(R. G. Wagner)

General Comments

I recognize the very difficult task the EIS team had in preparing this section. The data for young stand growth and yield in relation to inter-specific competition is not readily available, especially in relation to treatment efficacy and long-term projections.

The major weakness of Appendix A is the lack of information about the growth and yield projections for each alternative. This lack of information made it nearly impossible to evaluate the credibility of the conclusions. The main limitations were:

- Loosely defined or unstated assumptions
- No statement of inputs (site index, thinning, fertilization, etc.) in the runs
- Assumptions were often speculative and arbitrary when made
- No estimates of error or confidence--in fact, very misleading when stating 1-2% estimates of error on A-34 to A-42.
- Not enough summary output information from reported runs.

Providing the necessary information outlined above would be my first recommendation for improving Appendix A. These omissions provide the greatest obstacle to critiquing the document.

A major flaw is the basic premise of the analysis. It assumes some intrinsic difference among the tools of vegetation management, and a resulting effect on timber growth and yield. This is a problematic assumption that invalidates the whole premise of the analysis.

Sound vegetation management is conducted by first, defining the stand management objectives (e.g., 95% survival and a growth rate consistent with some standard for that site). Second, based on an assessment of vegetation conditions on a site, and an understanding of how the vegetation will likely limit (in quantitative terms) achieving the objectives, various treatment alternatives (including no treatment) to manipulate the vegetation (to a defined level) are considered.

Any level of vegetation manipulation is possible with one or a combination of the tools presented (chemical, thermal, manual, or

biological). Therefore, the management objectives (which have not been defined) can be accomplished with any of the tools. The EIS alternatives seem to combine, and therefore, confuse the objectives with the means to achieving the objectives.

Finally, a comparison of vegetation management tools, is based on the safety, environmental impact, cost, availability, etc. of all of the tools that can be used to accomplish the stated stand management objectives.

The growth & yield projections assume a mean tree response. Recent evidence from Petersen (1988) suggest that substantial differences in the distribution (or stand table) develop in stands where weeds are controlled or not. Much of the final stand yields and financial value will be linked to differences in the diameter distribution, not a mean tree response. Differences in distributions could not be calculated, however, the DEIS with the current scientific information, should state this limitation to the analysis.

Linear projections of exponential responses (Attachment 1-7) may not be reasonable. Nearly all available long-term data sets indicate diverging exponential trends in tree growth under low and high competition regimes. These linear projections probably underestimate competitive effects on stand development.

Specific Comments on Sub-sections:

Introduction:

PAGE A-2, PARAGRAPH 2. The first sentence states that "even with intensive management, it is unlikely that maximum yield (the full biological site potential) and the elimination of all competition will be achieved..."

This statement presents a rather poor statement about silviculture on USFS lands. The public is told not to expect site potential yields and that the cost of doing business is unlimited.

This introductory statement needs revision with reference to following points:

1. Maximum yield of what? The previous statement implies management for multiple resources. Is this maximum yield of all forest resources or just timber? The public might accept less than potential timber as a compromise to also producing something else. But I would think the



Figure 1. Distribution of data from thinned Douglas-fir plantations used to develop DFSIM. (Source: Figure 5 from Curtis et al. (1980).)

USFS would want to manage for at least a maximum biological potential of multiple resources, especially if funds are unlimited.

2. The time frame also needs to be defined. It is possible to have complete elimination of competition on some sites and a resulting maximum biological potential of plantation growth for a short time. This is currently done after prescribed burning or on grassy sites with good weed control. Complete vegetation control might be a reasonable silvicultural objective for the first year or so of plantation establishment, even though it may be environmentally and economically infeasible for extended periods during the rotation.
3. Not achieving site-potential yields follows further in the analysis when untreated control plots are set as the standard for comparison, rather than achieving normal yield table standards or at least survival standards. The EIS shows little evidence of what the forest management objectives are.

A-3, PARAGRAPH 4. The DEIS states that a reduction in cover will result in response from the crop tree. This statement is not true according to existing data. The negative hyperbolic curve describing the relation between yield and competition level indicates a substantial range of competition where little or no difference in yield results.

Assumptions:

A-4, PARAGRAPH 2 How can broadcast burning be considered available in the assumptions?

1. Alternative F presumes no burning
2. Prescribed burning is a vegetation management technique, yet is included in the vegetation management yield projections.

A-4, PARAGRAPH 3 Substitutability of vegetation management tools is assumed in the growth and yield analysis.

1. This assumption appears to be a contradiction since projecting yield differences among the tools is the basis of the analysis.
2. While this assumption may be fine in an academic framework, it is not realistic (as the DEIS points out) in practice. Therefore, when projecting yields based on methods, non-substitutability should be evaluated as part of the growth & yield projections among alternatives.

A-4, PARAGRAPH 5 "Vegetation control is geared toward conifer establishment (survival) and growth." This statement should include some mention of the growth and survival threshold concept which indicates that conifer growth and survival are not necessarily achieved simultaneously (Wagner et al. 1987).

A-5, PARAGRAPH 8. The DEIS does not describe the quantitative relation assumed between conifer seedlings and the volume of associated vegetation

A-6, PARAGRAPH 8 This paragraph describes an opposition to complete vegetation removal. But the USFS does this in practice all the time with prescribed burning. The DEIS should elaborate on the time frame involved. What are the positive and negative trade-offs the DEIS is trying to balance?

A-6, PARAGRAPH 3 A "degree of caution and conservatism is incorporated into the analysis." How? What?

A-6, PARAGRAPH 5. Implies that trees "grow through competition." Trees may grow above surrounding vegetation, but there is little evidence that they escape the competitive influence.

A-7, PARAGRAPH 1. Define "free-to-grow." This is an undefined silvicultural standard.

A-7 Literature is "erratic and conflicting"? If EIS team would review the literature presented in Stewart et al. (1984), a consistent trend in the biological principles would be revealed. Stand volume growth increases of 40 to 100 percent are common throughout the forest vegetation management literature. This statement should be elaborated or eliminated.

A-8, PARAGRAPH 1. "large statistical error terms...are adequate to display magnitude of change among alternatives."

Without confidence limits on projections, it is unreasonable to assume that reasonable comparisons of the alternatives can be made.

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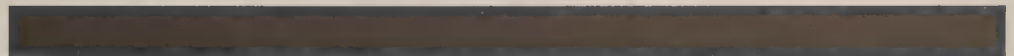
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Appendix I/A

Public Participation and Consultation



Section 6

Responses to Technical Reviews

Introduction

The University of Washington and Labat-Anderson, Inc., prepared the human health material for this EIS. They have each responded to the comments in the technical reviews. The comments and responses are listed in this section.

Richard A. Carchman, Ph.D.
(Virginia Commonwealth University)

Comment

In the overview section (p. 1, paragraph 2), a clear distinction should be made between noncarcinogenic and carcinogenic risk. In addition, avoid the use of jargon wherever possible. Include in the uncertainty sections:

- (a) rate of exposure,
- (b) vehicle effects, and
- (c) differences in xenobiotic metabolism and pharmacokinetics.

It might be useful to provide a more extensive discussion of risk per se, such that a lay person might be able to place in better perspective what estimation of risk assessment means.

On p. 2, paragraph 4, the last sentence might be fleshed out some more [for example, how much might these procedures exaggerate the risk (probably unknown)? What is the effects(s) of exaggerated (unrealistic) risks, perceived/real - public costs, etc.?)

Response

These additional uncertainties have been addressed in the overview of the risk assessments or in other sections of this appendix. The rate of exposure is incorporated in the uncertainties discussion. In addition, the differences in xenobiotic metabolism and pharmacokinetics are covered in the discussion regarding extrapolation factors from humans to animals.

Section 1 is an introductory section; therefore, an extensive discussion of risk is more appropriate where it is presented in Section 5 (risk analysis).

It would be impossible to quantify the exaggeration of risk in this analysis. This analysis utilized a conservative approach to err on the side of safety. The primary purpose of this approach is to decrease the potential risk to the worker and the public. This paragraph has been revised in the FEIS to more clearly define the purpose of overestimating risk.

The herbicides under consideration are the commercial formulations, not the pure material, and as such, it should be so stated. In the case of the animal studies, every attempt should be made to emphasize, where known, whether the pure or commercial grade material was tested. A sentence on pesticide (or more general) interactions should be added in this section. Tables and figures of chemical structures and physical/chemical characteristics would be helpful for the 16 herbicides.

A general statement regarding the contents of pesticide formulations was added. In general, basing the analysis on data obtained from testing the pure material is more reliable and conservative. Pesticide inerts that are on the EPA Inerts of Concern List are presented in Section 3. Because this section is a general overview, the discussion of synergism and pesticide interactions are more appropriately presented in another section. A discussion of synergism and pesticide interactions is presented in Section 5. Background statements are compiled on each chemical. These statements provide descriptions of each chemical's physical properties and structural characteristics.

On p. 4 (Hazard Analysis), add both rates and levels of exposure. Under chronic toxic effects, provide some reference or breakout of these. Under data gaps, refer reader to a section where these are identified in the EIS. Exposure analysis--change calculation to estimate doses. Risk analysis--not only should doses be compared but also the routes of exposure especially in the case of herbicides.

This discussion was designed to give the reader an overview of the risk assessment process. Detailed descriptions of chronic effects are presented in the individual chemical discussions. The doses are calculated based on estimations of exposure, as presented in the Dose-Response Curve figure. A reference to data gap sections for each individual chemical has been added to the FEIS. NOEL's used in the risk analysis are generally from oral exposure. Because oral exposure is likely to be the worst case exposure, comparison of oral exposure NOEL's to estimated human doses is a conservative approach.

Table 1 (p. 6) should state that these data are based/year. It is not clear to me why treating 100,000 acres/year "makes the worst case assumption." If it is based on adding all the figures in Table 1-1 together, then this should be so stated. I think wherever possible the basis for these scenarios should be indicated.

On p. 8--why are synergistic effects of mixtures mentioned without some mention that this would represent a worst case scenario. In addition, other attempts to estimate these kinds of risks simply assume additive (That is, noninteractive) effects (EPA).

The data gaps identified on p. 9 should be highlighted further. Item no. 6 (p. 10) is stated in such a way as to prejudice the reader. In addition, there are many different kinds of experimental designs that could be considered that might make these studies financially possible (For example, central composite, Box-Benken, etc.). The way this is written might suggest to a reader knowledgeable in this area that this statement reflects the thoughts of an uninformed writer.

This paragraph has been revised to more clearly define the worst case assumption.

The uncertainties of synergistic effects are discussed in detail in Section 5.

Data gaps are presented in more detail in Section 3. Item No. 6 has been revised to address these concerns.

On chronic feeding studies (p. 6, paragraph 2, section 3), the use of the phrase feeding studies (that is, administration of compound as an admixture in the animals' food) should be changed to more clearly indicate what is going on. In paragraph 5 of p. 6, the first sentence should more clearly state for what the tests results are used. You also might want to include somewhere in this document arguments relating to the relationships of these assays to carcinogenic endpoints. Caution should be exercised in using the terms mutagenicity and genotoxicity interchangeably.

On p. 16, Section 3 on Picloram, where it states, "EPA has determined that the positive study was insensitive . . .," does this mean to suggest that picloram is even more mutagenic? Rephrase to more clearly reflect the problems with the assay from which the EPA apparently has rejected the data.

On p. 24, paragraph 1, Section 3, was the least-squares linear regression procedure weighted or unweighted? If weighted, how so? On p. 24, paragraph 3 on Amitrole, why is there an exception here based on the use of a threshold? This should be clarified further.

The chronic toxicity study section has been revised and expanded for clarity. The mutagenicity assay discussion has been expanded considerably to better describe the relationship of these assays to carcinogenic endpoints. The terms genotoxicity and mutagenicity also are more clearly defined.

This sentence has been expounded to indicate the meaning of the insensitive mutagenicity study and why this particular test was inappropriate.

The EIS has been revised to incorporate cancer potency values that are derived from the multistage model only. The exception is the cancer potency value for atrazine, which was calculated by the Forest Service using the one-hit model. Amitrole is used as an example, and it is clearly emphasized that all chemicals were treated with a nonthreshold approach.

On p. 25 on Picloram, need to discuss and reference the significance and controversy of benign versus malignant tumors. This could be confusing with such an addition because the EIS states "picloram carcinogenicity . . ." You could argue another worst case scenario based on the assumption that these benign lesions could be premalignant, but without making such a point, this could be confusing.

A critical point is made and "buried" on p. 28, Section 3, paragraph 4--"The gap in the testing of the herbicides as formulated products, according to one view, gives rise to an inference that the environmental consequences, including hazards to human health, from using them are largely unknown." This point should have been included earlier under data gaps. Comparisons should be made between the pure and technical formulations in selected short-term assays to help address this key point.

On p. 7, Section 4, paragraph 1, how was the residue on vegetables estimated and the amount of material ingested? I assume that washing of the vegetable and skin or peel removal as well as convoluted surface areas of vegetables was also "included" in this calculation. Whatever was done in this case should be available for criticizing. For this entire section, what is the probability of these events occurring?

An addition to the general carcinogenicity discussion addresses the issue of benign versus malignant tumors.

This EIS does not consider the lack of testing on specific formulations as a data gap for a particular chemical. Each chemical is addressed separately, including kerosene (the inert contained in Esteron 99). An expanded data gaps discussion has been added for each chemical in the FEIS.

The specific details of residue estimation are not presented in this portion of Section 4 because it is intended to be an overview. Explanations of residue determination (that is, from field study data) are presented in a subsequent portion of this section. Since these analyses assume that worst case for each scenario, no allowance for the cleansing of produce were made.

P. 11, Section 4, I think that a very clear statement (including data) should be provided to allow for a better appreciation of the relationship between exposure/dose with different levels of worker protection. I indicate this in part because of the data in Table 4-4. I may have misunderstood the information included in Table 4-4. See Lavy et al. study wherein it appears that individuals with protective gear seemed to have higher average doses than less protected individuals. Some statistical analysis of these data might be additionally revealing.

The use of the word "realistic" to describe a situation less extreme than a worst case (for example, dose) could be misleading for those readers who do not delve into the definition (that is, realistic).

On p. 21, Section 4, paragraph 3, could you refer the reader to some reference as to the computer program? Are there competing programs that give different results? What other assumptions are made by this computer program?

An expanded explanation of this table has been added to Section 4 in the FEIS.

The term "realistic dose" is clearly defined in the EIS. The word "realistic" is used because it implies that the scenario could occur under normal conditions.

The computer program used merely reflects the exposure computations described in the exposure analysis section. It was written specifically to perform these calculations.

On p. 22, Section 4, paragraphs 1 and 2, is the consumption of 400 g of peas or berries and 1 liter of water on a daily basis? Why does 400 g have (0.9 lbs) and 1 liter have no equivalent? It is a minor point but should be consistent. In addition, the consumption of contaminated food that will be consumed following cooking--does that also assume no loss of herbicide? This should be so specified. This document also should again indicate where the assumption of daily fish consumption was obtained --same for other consumable items. Table 4-8 should indicate whether these are daily doses.

In Section 5, p. 21, paragraph 3, what would the actual differences be between estimated cancer risks using the threshold and nonthreshold models? Do you think that EPA's model use is really less appropriate?

It appears from the way in which amitrole is discussed that (p. 25, section 5) it could (or should) be a good candidate for removal from use. How does a nonexpert in risk assessment use this information in "Cancer Risk to the Public" to evaluate the significance of this section? Table 5-15 does go a long way toward dealing with this issue(s). Do you think it would be worthwhile to have the concepts that this table conveys as part of an executive summary?

Sources of the assumptions used in human consumption estimations were added to the text. In addition, sample calculations were added to the section for clarity.

The use of the threshold model is less conservative than a nonthreshold model. It is not the intent of this discussion to infer that EPA's model is inappropriate. The nonthreshold approach is consistent with the conservative nature of this document.

Your concern is well-founded and will be addressed in the executive summary.

On p. 30, Section 5 (Synergistic and Cumulative Effects), why must synergistic effects result from simultaneous exposure? I would think that time also should be considered as a significant dependent variable. What do you mean by synergism?

The conclusion that synergistic effects are unlikely to occur results from the remote possibility of exposure during two separate projects, rather than the likelihood of synergism occurring between two pesticides. This section has been revised in the FEIS to present a more precise explanation of synergism and a more thorough discussion of the uncertainties associated with synergistic effects.

Frank N. Dost, DVM
(Oregon State University)

Comments

Page 1, paragraph 3--Other forest workers than applicators may be subject to occupational exposure.

Page 1, paragraph 4--"Safe" levels are not established in a laboratory. The sentence beginning on line 2, "First effects," should be rewritten. In the following sentence, the word "additional" is used, and no prior safety factors have been discussed. NOEL's are not safety factors.

Page 2, paragraph 1--I would suggest defining "safe" somewhere in this part of the document. I prefer to define safe, or safety, as the practical certainty that no harm will arise from a given use (or dose). There are other similar conventions.

Page 2, paragraph 2--Somewhere in the front in both volumes, there should be a very bold statement of conventions, including the caveat expressed here, that the analyses in the document are based on estimated single doses or few human doses compared with daily doses given animals over periods as long as a lifetime. These are well described in various places in the DEIS and should remain, but the condition should also be stated in a place of high visibility.

Responses

This sentence has been revised to indicate a broader definition of "forest worker."

The word "safe" has been replaced with a more appropriate term.

The word "safe" has been replaced by a more appropriate term.

This issue has been addressed in the appropriate portion of the EIS.

Page 2, paragraph 3--This paragraph is quite unclear. The first sentence should indicate that because it is not known that there is a threshold, and because it is biologically plausible that there may be no practical threshold, it must be assumed that there is none. Therefore, we must assume for purposes of regulation or risk assessment, that any small dose of a carcinogen has some probability of a carcinogenic effect. (See also Section 3, page 4).

This paragraph, as well as the comparable discussion in Section 3, has been revised to address your concerns.

Page 2, paragraph 3--The cancer potency can be defined a little better. It is a projected probability that cancer will be caused at a standard dose rate, usually 1 mg/kg/day over a lifetime. It is used by a linear comparison with measured or estimated doses in the field averaged over a working career or a lifetime. The "probability of developing tumors at increasing dose levels" is an expression of the dose-response curve, which is used all or in part to define potency.

The definition of cancer potency has been revised in the FEIS.

Page 3, paragraph 2--Hazard, risk, and exposure are not defined in the glossary.

The definitions of hazard and risk have been added to the glossary of the FEIS.

Page 3, paragraph 2--It seems to me that hazard analysis is a determination of the nature of the effects, which includes both the kinds of responses and the dose response relationship. Why is chemical structure necessary to hazard analysis? Does it direct additional research, or is it used to estimate risk?

Data on chemical structure is important to identify characteristics of chemicals that are relatively unknown. Identification of chemical structure, metabolites, and degradation products can facilitate the search for current literature in data bases.

Figure 1--I must question whether LD₅₀ has sufficient utility to list separately. It is subsumed under the first bullet, where it may be desirable to say, "including acute responses."

Page 8, paragraph 3--Estimated average daily exposure should be explained more completely. This statement leaves an impression that everyone is exposed continually, and does not make it clear that even a single dose would be averaged over a lifetime for the risk assessment.

Page 8, paragraph 4--There is no quantitative risk assessment for mutagens. The convention used in this document, that cancer risk reflects mutagenic risk, is quite satisfactory lacking any better method.

Page 8, paragraph 5--The sentence on synergism is probably technically correct, but it tends to imply that there is actually a fair amount of useful direct data to appear later. In the rest of the document, the issue is discussed in the sketchiest of terms where it arises at all.

In this analysis, the LD₅₀ value for any particular chemical is used to classify the chemical based on toxicity. These classifications are particularly useful because they provide a means for comparison of the given chemicals.

This discussion has been revised to address your concerns.

This discussion indicates that there are various mutagenicity assays that may be used to assess the mutagenic potential of a chemical. A detailed discussion of the weight-of-evidence approach is presented in Section 3 of the FEIS.

This sentence has been revised for accuracy.

Page 10, paragraph 1--There should be some definition of just what the gaps really are and why they are important or unimportant to the DEIS. In the usage of this document, are they simply areas where the EPA has required additional data, or are they areas in which there is a great deal of information already that does not fit the pattern required by the EPA, or is there just an absence of usable data?

Section 2, page 9--The issue of zero wind should be addressed. No application near a sensitive site should be made unless there is an active wind away from the site. There should be no aerial treatment near sensitive sites in dead air.

Section 3, page 2, paragraph 4--The words "metabolize" and "metabolism" should be in the glossary. Differentiate between metabolism of pesticides and metabolism of energy substates. For this purpose, it is not necessary to discuss metabolism of hormones, neurotransmitters, and other endogenous non-energy producing substances, nor is metabolism in the sense of biosynthesis pertinent.

Page 2, paragraph 5 and page 3, paragraph 1--These two paragraphs are not well separated and probably ought to be redrafted. They should include a clear discussion of dose-response relationships. In fact, it probably would be best to include a separate paragraph on dose response.

The discussion in the introductory portion that you address is a general discussion appropriate for this section. A more detailed discussion of data gaps for each chemical is presented in the FEIS.

This additional point has been incorporated into this discussion in the FEIS.

These terms have been added to the glossary in the FEIS.

This discussion has been revised in the FEIS to address your concerns.

Page 3, paragraph 3-- Parenteral dosing does not bypass natural protective mechanisms. In fact, the result may be to move some of the material to the liver through the portal circulation without the moderating effects of the gut content and dilution in gut volume.

Page 3, paragraph 4--The weight of the animal alone will not give a conversion. A rough approximation can be based on standard consumption data for a given species and weight, or food consumption is monitored directly. The latter is usually done to account for unusual relations between weight gain and intake and to provide accurate data on dosage per unit weight. The procedure for converting dietary concentration to dosage ought to be presented. Elsewhere in the document, conversions are stated, with reference to the USDA 1984 background document, but with no mention of the method.

Page 4, paragraph 1--Acute toxicity studies are used for much more than determining LD₅₀'s. The LD₅₀ is not useful for much more than determining doses to be used for single dose studies and as a first step in rangefinding for longer term work.

Page 4, paragraph 2--This is essentially a one-sentence paragraph that is rather unclear. Among other things, no-effect doses are developed for studies of all lengths.

This discussion has been revised to address this issue.

Conversion factors have been identified in the appropriate portion of Section 3 in the FEIS.

This discussion has been expanded to address your concerns.

You are correct. This discussion has been expanded to address your concerns.

Page 5a--Threshold is not quite correct in this usage. It is the point at which a response first appears on the dose response curve. As pointed out in Casarett and Doull (3rd ed., p. 19-20), it cannot be determined experimentally. An infinite number of experimental points or extraordinary good luck would show it directly in the data; otherwise, it is derived statistically.

Page 6, paragraph 2, line 2--Not necessarily. For nongenetic effects, very little additional information appears after the first 3 or 4 months of testing.

This concern has been addressed in the text to further explain the diagram.

There are many effects (particularly histopathologically) that are visible only after long-term exposure. In addition, hematology and clinical chemistry data over a chronic exposure period are much more reliable.

Page 6, paragraph 5--Mutations are most easily visualized as physical changes because of the nature of the molecule and the nature of the change (deletion of single or multiple bases, insertions, broken chromosomes, etc.). However, almost all interactions are chemical reactions that result in adducts, in bases being deleted from or added to the sequence, or in incorrect repair of damage.

Table 3-2--I would suggest rounding such numbers as the atrazine LD₅₀. Numbers presented with this apparent precision give a false impression of the ability to make measurements in biology. Somewhere it is necessary to make certain that readers understand such numbers to be merely the creatures of arithmetic treatment of measurements that themselves have but modest precision. Perhaps it would be useful to include the standard deviation and an explanation that this is simply a number that has a high probability of really being within a certain range of values.

Page 8, paragraph 5--The description "greater than" is used frequently and should be explained as relating to the highest dose testing, and limited or no effect at that level.

Perhaps "structural" would be a more appropriate word. The word has been replaced in the FEIS.

These values were derived from laboratory data and reported in this literature as they are presented in the table. It is not possible to calculate a standard deviation in the absence of actual data.

The table has been revised in the FEIS.

Page 9, paragraph 2--Kidney effects are systemic. I do not recall kidney among the list of organ systems or functions to be considered separately from general systemic effects.

Speaking of that issue, I do not agree that neuro- and immunotoxicity should be set apart as nonsystemic. Every chemical, however non-specific, will hit some site before it affects any other. That does not make the effect specific to that organ or function unless the response is clearly at rather low doses relative to onset in other organs, or the effect is so characteristic that it must be set apart. The neuromuscular effect of 2,4-D is probably specific enough to identify as such, even though it is quite rare in humans and requires high doses. It is not quite the same as the myotonia seen in rodents.

Page 9, paragraph 2--This implies that 2,4-D is not a teratogen, which is not correct. Other species do show such effects; it does not matter that the EPA may not consider the other studies definitive. There is such a mass of data that shows positive responses that they cannot be ignored. The fact that the dose response indicates that the prospect of effects on humans is very remote is a separate issue.

The subject of this comment is unclear and cannot be identified.

This discussion has been revised to include more detailed evidence and explanation.

Page 10, paragraph 4--How does this NOEL of 2.5 mg/kg/day square with the fetotoxic NOEL, fifth line from bottom? In my view, the reproductive or fetotoxic NOEL ought to be used as the working NOEL when it is the lowest number, except when the species is clearly inappropriate. Much of the time, the effect is a function of maternal rather than fetal effects, even when no maternal toxicity is detectable. Consequently, it implies some kind of effect that may well occur in both males and females, unless it is highly specific to the female reproductive or endocrine apparatus or sex differences in drug metabolism.

Page 13, paragraph 2--The inappropriate use of dog data to establish NOEL's on triclopyr or any other organic acid is well described here. The data and explanation should be mentioned, and no estimate of risk should derive from these findings.

Table 3-4--My scorecard on mutagenesis shows a much higher ratio of negative to positive. In any case, it is a very weak mutagen, and within your rules for this exercise, it is appropriate to assume it to be mutagenic for this exercise.

Page 20, paragraph 5--The Donna et al. work is so poor it should never have been published, and it ought not to be used.

This section has been updated and revised to include a more detailed discussion of the basis of NOEL selection.

The value derived from the dog study was not used to quantitatively assess the teratogenic potential of triclopyr.

2,4-D is acknowledged as a weak mutagen according to existing evidence. In addition, 2,4-D was considered a mutagen in this risk assessment.

The reference to this study in the text has been omitted.

Page 23, paragraph 5--The newer data on triclopyr is in the hands of EPA. Shouldn't they be incorporated? A 228-day dog study would probably not detect even a virulent carcinogen.

The new data on triclopyr have been incorporated in the FEIS.

Page 23--The cancer potency material should be at the beginning of this overall discussion, perhaps in an introductory page or two discussing concepts employed in estimating carcinogenic effects and risks. The idea of the 95-percent upper bound needs special attention in such an explanation. There is enough detail about cancer and cancer studies in this portion of the document that there may be a need to discuss the criteria under which laboratory carcinogenesis studies are judged. This would be particularly pertinent in the cases of glyphosate and 2,4-D.

This section has been revised in the FEIS.

Figure 3-2--This graph suggests that the range of likely human doses is far higher than realistic. In effect, the graph with no numbers, says that human exposures produce doses as high as one fifth as great as those that produce cancer in the laboratory. Even though it is not the intent of the figure, someone will conclude that it shows real information about the herbicides proposed for use.

A better explanation of this graph has been incorporated in the FEIS.

Page 28, paragraph 3--I believe it is known that the petroleum distillate in question is kerosene, used as a diluent for ester formulations.

This discussion has been revised to indicate that kerosene is the distillate that is being addressed.

Page 28, paragraph 4--
Overstating the risks
associated with the herbicides
does not make the risks of the
inerts insignificant, because
any hazard that might be
involved is almost certainly
of a different nature. The
draft is correct in stating
that only one inert is listed
as requiring attention. The
treatment of this issue in
both volumes should be brought
into alignment. I have
commented on the inappropriate
discussion in the main DEIS.

Section 4, pp. 7-12--This
section and the dependent
Attachment B have given me
more frustration than any
other part of the project. I
have tried to work my way
through one of these
calculations just to see
whether the approach is
logical, and I cannot make any
progress. Whether I am obtuse
or the discussion is badly
organized, I do not know. My
initial problem is identifying
the source of the figure on
the last line of page 9--70
pounds of 2,4-D applied on
average per applicator.
Overall, I think the process
of estimating these exposures
could be laid out in a much
more usable form, so even I or
a supervisor on a project
could use the process to
estimate exposure in some
specific situation.

This discussion has been
revised to address your
concerns.

This section has been expanded
to further explain the
calculations, as well as the
sources of values used in the
calculations.

Page 2, paragraph 2, line 5--
This is an incorrect
statement, unless the
immediate environment is
clearly defined as limited to
surfaces from which absorption
can take place. Material
trapped on clothing away from
skin, bound to soil under
foot, or adsorbed to pants in
the general vicinity does not
constitute exposure.

This statement has been
revised for clarity.

Page 6, paragraph 5, lines 8-
10--I just do not understand
what this sentence means.

This statement has been
revised for clarity.

Page 8, paragraph 3--Should
there be a section at the
beginning of the chapter
discussing the methods that
underlie determinations such
as this? How will the lay
reader be told that for some
chemicals everything that goes
into the body emerges intact
in time for the reader to
understand this kind of
approach. It is described
reasonably elsewhere, but I
think it needs to be explained
on page 2 under exposure and
dose. This is another example
of the problem of information
distributed far and wide in
these books.

You are correct. This
information also has been
incorporated into the
introductory portion of this
section.

Page 8, paragraph 7--As I read
this, it implies that the
entire width of that right of
way is treated, gravel and
all.

This statement has been
revised for clarity.

Page 9, paragraph 1--When using a standard 10-percent absorption factor, which is appropriate when lacking any information, it should be pointed out that skin absorption is largely governed by fat solubility, and as a consequence, for substances that are not quite lipophilic (fat-soluble), skin permeation should be very low, as is the case with picloram. It is doubtful that even the ester formulation of triclopyr moves at a rate approaching 10 percent. Data on human absorption of triclopyr now exist, and I presume they have been made available.

Page 9, paragraph 1--According to the review on amitrole in USDA 1984, page Am 31, "in the absence of data to indicate otherwise," the absorption rate of amitrole is considered to be 10 percent. Which factor is used here?

The dermal absorption value of 1.6 percent for triclopyr has been incorporated in the FEIS.

An appropriate reference has been added to the text for the dermal penetration rate of 0.1 percent for amitrole.

Page 9, paragraph 1--A figure of 0.48 percent for picloram absorption is also used, with reference to Lavy 1984. The 1987 publication of that work in Environ. Tox. Chem. 6:209-224 shows no such figure, and in fact, in the abstract, the authors say, "If equal dermal penetration of 2,4-D and picloram is assumed, ...", indicating that they do not have a penetration rate. It is likely that an estimate can be made from those data and some assumptions, but no indication of that exercise is evident. Furthermore, the question of picloram dermal penetration through human skin has been published, and it is indeed very low (Nolan et al., Tox. Appl. Pharm. 76:274, 1984) Why neither Lavy et al. nor the authors of this document did not use those data is curious.

Page 22, paragraph 1--The time frame of the exposure should be specified. In the case of peas, what accounting is made of the loss of the pods and herbicide deposited thereon?

Page 22--In considering exposure of animals, I am willing to accept that an animal will move in such a way as to expose most of the body surface, even though drift is unidirectional. It is in error, however, to make the same assumptions for access of surface deposition to the skin. Because of the enormous surface area of the hair, and its natural repellency, a rather small fraction will reach the skin, where absorption is probably similar to that of the human skin.

A conservative dermal penetration figure was derived from Lavy's data based on the known dermal penetration of 2,4-D.

The approach of this EIS is very conservative. Therefore, it is assumed that the individual would consume the entire vegetable immediately after application.

Again, because of the conservative nature of this analysis, the entire body surface area was assumed to be covered.

Page 22, paragraph 4--The figures for acquisition by grooming are apparently assumptions, for which I can locate no basis. There will unquestionably be some removal by grooming, but the calculations for deposition, absorption, and grooming should be integrated, and the basis stated, even if it is a desperate guess. Guesses and dart-throwing are not necessarily improper if no other approach is possible, but the method used must be stated clearly.

Page 22, paragraph 5 -- This is not the way the issue was discussed in the background statement. The assumption in that case was that 10 percent of the dermal and oral exposure would be assimilated and that it would be retained, which in itself is very conservative. That is different from 10 percent of the total dose. Both assumptions are too conservative, because skeletal tissue retains relatively little of such agents, while residues in liver and kidney are relatively high. Those latter differences are admittedly difficult to factor in. The background statement assumed consumption of meat for 2 days. In this paragraph, we are not told how many days of consumption are assumed. In this particular area of secondary dosage, there are a number of inconsistencies with the introductory discussions in the background statement, and they should be reconciled with this document, or differences should be explained. It is likely that the background statement will be considered

This section has been revised in the FEIS to more clearly define the logic of these assumptions.

The methodology for determining surface-to-weight ratio and tissue retention of the herbicides was developed independent of the methodology in the background statement. These changes do not create significant differences in the conclusions regarding human health risk.

as being associated with this document, because it was constructed as part of the general effort of which this DEIS is also a part. Some commonality should be established. Among other things, deer in this DEIS have a surface-to-weight ratio of 0.039 sq m/kg, compared to 0.029 for the deer in the background statement, which may be overfed. These numbers may be compared with the figure for a small human on page 26, paragraph 3 of 0.016. The shape of a human is not different from that of a deer to make that much difference, unless the person is truly rotund.

Page 24, paragraph 3--The use of Popendorf's insecticide model is inappropriate for use with herbicides. Fungicides and insecticides are for the most part intended to remain on the surface of plants, to either contact or be consumed by the pest of concern without affecting the plant. They are therefore much more available for skin deposition. They may also mobilize in moisture, such as the dew of early morning. In fact, Popendorf distinguishes between low, damp and high, dry foliage. If they were absorbed in appreciable quantities, the residues on edible products also would be greatly amplified and would not be removed in processing or washing. The Popendorf model relates specifically to organophosphate insecticides and has some potential for application to others. However, an important part of the model includes levels of exposure that will cause a decrement in cholinesterase

levels. In other words, itPopendorf's model was used to estimate exposure only. Other aspects of this model that relate specifically to organophosphate insecticides were not utilized in this analysis. The text discussion that references this model has been revised to indicate which aspect of the model was used.

responds to exposures and consequent doses that have exceeded the NOEL, not just an arbitrary 100-fold safety factor. That is hardly consistent with the conventions established for this document.

The work of Gerry Stephenson's group at Guyelph shows empirically that once 2,4-D is applied, it is very difficult to remove (Thompson et al., Pestic. Sci. 15:353, 1984). Although data for most other herbicides are not available, similar behavior should be expected for those other than desiccants; otherwise, the herbicide would be ineffective. Metabolically active herbicides cannot function without entering the plant, and as a practical matter, if the herbicide does not either bind tightly to surfaces or enter the foliage, it will wash off and be ineffective. There at least should be a discussion of this behavior in the discussions of mitigating factors.

Page 27, paragraph 2-- Metabolism has nothing to do with exposure.

Table 4-9--I think logic ought to be allowed to prevail here. By day 30, no berrypicker will go near an area that has been sprayed, let alone pick the berries. A hiker will shun the area as well, because the values sought will have been depleted.

This point has been clarified in the FEIS.

The reference to metabolism has been removed in the FEIS.

This is most likely the case; however, the possibility that the pesticides were ineffective was considered.

The ADI and MOS are both based on quality of data and on the NOEL. The only real difference is that with the ADI an outsider or an agency is incorporating some kind of judgement and telling us what is acceptable. When using an MOS, we are simply informed of the difference between field dose and NOEL.

Section 5, Page 1, paragraph 2--This passage is confusing. There is a world of difference between the highest NOEL and the lowest NOEL. In the following sentence, in reference to noncarcinogenic effects, this could be read to mean the other effects caused by a carcinogen.

Page 2, paragraph 1--Risk is misused here and in paragraph 4 on this page. An MOS does not define risk; there is no practical difference between an MOS of 1,000 and one of 10,000, or for that matter 100. The MOS does not predict an effect or probability of effect. No one knows what an MOS of 50 means, but because we do not want to find out, we use an MOS of 100 or 10,000 as a standard.

Page 2, paragraph 3--This sentence is curious, as are the tables that relate exposure level to LD₅₀. We are not properly interested in fatal effects, and I recall no place in this document where risk of fatality is discussed, the tables notwithstanding.

Page 3, paragraph 3--Some of these chemicals are listed as questionable carcinogens elsewhere. Use of "positive" needs explanation.

This discussion has been revised to more clearly differentiate between the ADI and MOS.

This statement should refer to the lowest NOEL only. It has been revised in the FEIS. The word "noncarcinogenic" has been replaced by "systemic" in the revised FEIS.

This section has been revised in the FEIS to address your concerns.

We acknowledge that this is an extreme comparison; however, the use of this comparison is defined in the discussion. This value is to be used for a reference point for doses that exceed the NOEL.

This statement has been revised for clarity in the FEIS.

Page 5, paragraph 7--In judging the residues in deer and secondary exposures therefrom, it also would be useful to also consider the data developed by DOW in experiments with goats given triclopyr. Very roughly, about 1 percent of a dose of 0.2 mg/kg remained in tissues at the end of 10 days, assuming conservatively that all tissues in which no triclopyr could be detected contained levels at the detection limit of about 0.003 ppm. It should be expected that 2,4-D would behave similarly. These data are part of the proprietary package, but they were provided to a citizen group through an FOI action in Idaho several years ago. They should surely be available to the Forest Service.

Page 6, paragraph 1--This is a very important statement, because it brings reality into the analysis. As discussed elsewhere, this concept should be prominently stated early in the document. I would like to see it in bold type or with another kind of emphasis, because the calculations are really intended to see how bad it could get if each possible adverse condition could be met. Some of the statements later on the page, taken alone, can imply that there is a realistic prospect that the worst case depicted can occur and that residents will be subjected to exposures that will lead to low margins of safety and that they may suffer acute toxic effects.

Thank you for these additional data. The predicted levels used in this section are the highest values available in current literature.

This statement is incorporated in the appropriate section prior to the discussion of margins of safety for worst case scenarios. There are many other important points in this analysis; therefore, it would be inappropriate to emphasize this one with bold type.

Page 6, paragraph 2--I think the use of amitrole needs a careful examination. As a carcinogen, it is really of no consequence in the context of use in forestry; I agree with the EPA that it is almost certainly a nongenotoxic agent. The effects on thyroid function are not trivial, however. Measurements in humans show that careless and poorly clothed applicators using amitrole will experience deficits in thyroid function within a few days of first exposure. These are reversible effects early on before compensation begins, and a single exposure probably does not matter. However, an individual with thyroid dysfunction or other metabolic insufficiency could be at risk of acute illness. If the chemical is sufficiently useful, special training and practices will eliminate the hazard. Amitrole represents an odd situation in which home users may be at less risk than people working with it regularly.

Page 6, paragraph 2--I question strongly that dilute 2,4-D in chronic exposure will result in neuromuscular disease. The cases of peripheral neuropathy on record have almost exclusively resulted from exposure to concentrated material in a single large dose. I am aware of assertions that dilute material under field conditions has caused such effects, but no reliable documentation has been produced. The excretory clearance of 2,4-D is sufficiently rapid that only a massive dose can produce the target loading necessary.

This discussion of amitrole and possible thyroid effects has been expounded in the FEIS to address this concern.

An additional qualifying statement has been added to emphasize the conservative nature of this sentence.

Page 6, paragraph 2--As the document points out later, the calculations on triclopyr are based on dog data, which are is acknowledged several times as not being representative. The human renal excretion mechanism for organic acids is very efficient, and the assessment should be based on different numbers than those arising from dog work. However, this demonstration may properly lead to some mention of the consequence of heavy exposure of people with renal or hepatic disease for most or all of the compounds.

Page 6, paragraph 3--Why does weight loss relate to potential stomach problems? Weight loss and similar indices are entirely non-specific, unless a particular condition is identified that might reasonably lead to the weight loss. Is a metabolic deficit involved? Is GI absorption compromised? Appetite failure can result from anything from CNS depression to peripheral pain.

The issue of sensitive individuals (that is, individuals with renal or hepatic disease) has been addressed in the FEIS.

You are correct. This statement has been removed from the discussion in the FEIS.

Page 7, paragraph 3--I think an essential fact has been neglected here. A bushel of berries frozen and eaten every day without washing will not produce a cumulation of dosage with illness. The statement implies ("might feel quite ill") that there is a good chance that sickness will occur. That conclusion cannot be supported. If I recall, the daily dose of 2,4-D in this situation would be 0.048 mg/kg. If this amount was cumulative, the total dose would be 30×0.048 or 1.44 mg/kg, which exceeds the chronic daily NOEL, although not very far. If parallelism between experimental animals and humans is assumed, that amount in a single dose may or may not have some effect; it is not likely, given that the NOEL is set from a chronic daily dose. This statement has been revised in the FEIS to indicate that this exposure could potentially produce mild systemic toxicity. If we assume for the moment uniform distribution in the body and a disposition half-time of 15 hours, at steady state, the concentration in the body will be about 1.5 times the maximum concentration on day one, and that maximum would be achieved by about day four or five. Because ingestion probably takes place once a day, the tissue concentrations on a whole body average basis would vary from about 0.025 mg/kg prior to eating to about 0.075 mg/kg (ppm) immediately after consumption, assuming instantaneous distribution. For the sake of this argument, this concentration may be likened to a daily maximum point dose of 0.075 mg/kg,

which is also far enough below the chronic NOEL that an adverse response would be unlikely. The same mechanics have to be applied to game animals as well. For fish, the BCF assumptions accomplish the same end.

Page 13, paragraph 7--The identity of combustion products of triclopyr are generally predictable on the basis of the analysis I did for Bonneville Power a few years ago, although no proportions or quantities can be derived from that hypothetical approach. I would suggest consulting Bush et al., Arch. Envir. Contam. Toxicol. 16:333, 1987, for more specific data. Most of the authors are Forest Service scientists. The article deals with 2,4-D, dicamba, picloram, triclopyr, and dichlorprop on firewood.

Page 14, paragraph 4--Remove "the." There are many other PAH's that are probably carcinogenic, but there are insufficient data to analyze. The added risk from those compounds is probably not significant. Understanding of environmental behavior of PAH is not complete, but it is likely that their impact is less than simple dilution calculations would imply. Work is in progress to estimate risks associated with aldehydes, carboxylic acids, and acrolein in forestry smoke. Carcinogenic risk is probably negligible, but there is a very real potential for acute respiratory effects from this family of products.

Thank you for this additional reference. Information was incorporated as appropriate.

This discussion has been expanded to address uncertainties and additional concerns of adverse health effects.

Page 16, paragraph 4--I think this statement is too strong. It is likely that this level of exposure has been experienced many times, given the casual attitude of some workers, but the reports of impact are not there. It is better to describe it as having an MOS that is too small, and therefore requires tighter discipline and perhaps a different local decision process.

Page 18, paragraph 2--This is an important paragraph and ought to be emphasized in some manner. What relation is there between work protocols and worker instructions, and the EIS?

Page 18, paragraph 4--It seems highly unlikely that anyone would drink this water. Self-mitigating incidents should be given their due. Undrinkable water and defoliated berry patches are examples.

Page 21, paragraph 4--No study can show conclusively that a chemical is not carcinogenic. In the discussion of synergism, which seems quite reasonable, it should be emphasized that these kinds of responses are not a result of chemical interactions between chemicals, but are separate physiological responses in the body that may add or subtract. They will be expected to be threshold dependent as well.

Page 32, paragraph 5--The word "potential" ought to precede risk.

This statement suggests possible effects from exposure and is consistent with the cooperative approach of this analysis.

Again, the conservative approach of this analysis estimates the worst case, which does not account for worker protocols and protective clothing.

Although it is unlikely that individuals would consume water and berries in this situation, the worst case analysis assumes that this is a possibility.

This discussion has been revised to better define synergism in the FEIS. Explanation of assumptions and overestimation of risks are clearly indicated in this discussion.

The word "potential" was added to this sentence for the FEIS.

Attachment A, page 1--Amitrole. Is this material lifted directly from EPA documents, or has EPA (1985a) been further summarized?

Page 1, paragraph 2--Dose range is given but no dose response. A minor point, but the statement suggests effects at 0.1 ppm and limited cell toxicity at 100 ppm, which is interesting, even though mutagenicity data cannot be incorporated into a quantitative risk estimate.

Page 3, paragraph 7--As noted earlier, it does not take a prolonged period for sloppy applicators of amitrole to demonstrate thyrotoxic effects. The description of the sequence of events is well stated, however.

Page 5, paragraph 2--Atrazine study results: the dose response should be included here. This has been a rather surprising finding and should be detailed a bit better.

Page 6--Review on 2,4-D ought to be updated. The recent report by an expert committee of the Ontario Ministry of Environment will be found to be very useful.

As indicated, the information is presented as it was summarized by the EPA.

This statement has been revised for clarity in the FEIS. Four studies are discussed in which doses ranged from 0.1 to 100 mg/ML.

Thank you for your comment.

The discussion has been revised to address these concerns.

This review has been updated in the FEIS based on the most current available literature.

Daniel Wartenburg
(University of Medicine and Dentistry of New Jersey)

Comment

Response

In considering Quantitative Risk Assessment (QRA) as a tool in setting management priorities, one must recognize its limitations. QRA is a methodology designed to evaluate the consequences of a variety of likely outcomes. For events that can occur routinely, the methods result in a set of numbers indicating the relative frequency and severity of harm. The methods, however, are notoriously unreliable for catastrophic and unusual events.

We acknowledge the uncertainties involved in quantitative risk assessment and address these issues in the EIS. The EIS takes a conservative approach and incorporates worst case scenarios in the analysis. Therefore, the analysis does not ignore the possibility of "catastrophic or unusual events."

In the introduction, the authors make the interesting point that human health response may vary depending on whether exposure is acute or chronic. Certainly, this makes sense. However, it is not incorporated into the evaluations in a quantitative manner.

Margins of safety are calculated based on the no-observed-effect-level for chronic exposure. Because the no-observed-effect-level is at a lower dosage than would be expected to induce acute toxicity, it is not necessary to incorporate acute toxicity into these calculations.

The exposure evaluations provided are very limited. The authors claim that they are a worst case scenario, but certainly more adverse conditions could be imagined. No justification is given for the claim of worst case. There should be some qualification of the statements in this appendix.

This EIS examines a range of possible scenarios including worst case. The worst case scenario was developed through a series of conservative assumptions that tend to overestimate risk. These assumptions, as well as the uncertainties involved in predicting exposure, are detailed in this document.

The one-hit model for low dose extrapolation is used in the DEIS. This is surprising in light of the EPA's recommendation of using the linearized multistage model. More discussion of this choice and the impact of using alternative models should be presented. A discussion of the variability in low dose response estimation based on the choice of model would be useful in characterizing the variability and unreliability in risk estimates and in supporting the notion of comparative rather than absolute evaluation of risk. Some investigators have shown that some chemicals have supralinear dose response curves. In such an event, the one-hit model is markedly nonconservative.

The exposure section of this document is the weakest point of the entire analysis. Use of terms like "routine-worst case" suggest inappropriate considerations of adverse situations. If something is routine, it cannot be worst case. The scenarios appear to be meant to portray adverse conditions but then are mollified so as not to be too bad. Certainly, worst case and accidental situations should be given ample consideration. Public exposure estimates are not based on field studies but rather on modeling exercises. Again, are these likely to lead to "worst" case? I doubt it.

Thank you for your comments. The revised version of the EIS uses cancer potency values that were derived using the multistage model.

Again, conservative assumptions are made throughout the analysis. These assumptions are incorporated into the modeling exercises. In addition, aerial drift data from field studies were incorporated in the modeling exercises.

Wartenburg

The use of a safety factor of 100 is suspect. While this number is used routinely in toxicology, it is explicitly assumed that this number represents a worst case for all chemicals. But, chemicals with steep dose response curves ought to be modeled with even larger safety factors (1,000, 10,000, etc.) to prevent accidental problems. If a single safety factor is used instead of a complete dose response curve, one must allow for an ample margin of safety for all chemicals in all situations. One must allow for variations among chemicals and for differential sensitivities to different agents. A safety factor of 100 does not provide this level of assurance. I also doubt that this factor accommodates the most sensitive subpopulation. Risk assessments should.

Risk comparisons of herbicide use with electrocution, motor vehicle accidents, and the like (Table 5-15) are wholly inappropriate. Risks among alternatives should be compared, but these risks should not be compared with totally unrelated activities. The choice confronting forest managers is whether to use herbicides or other management options. Knowing that herbicide use is safer than skydiving begs the issue of how best to manage a forest.

The use of safety factors was based on existing data for each chemical. Because a number of conservative assumptions were used in the analysis, a safety factor of 100 was considered an appropriate factor to account for variability among species and sensitivity within the human population.

Because the audience of this document is not composed entirely of the scientific community, these comparisons are necessary to facilitate a better understanding of the risk analysis.

Francis W. Weir, Ph.D.

Comment

In many areas, this DEIS is overly detailed to the extent that pertinent environmental information and analyses are buried. Material that is not critical to subject development should be placed in an appropriately referenced appendix.

The practice of overestimating toxicity, exposure, and risk is not scientifically acceptable, especially because there is no way for the reader to quantitatively measure the degree of overestimation.

The authors of the DEIS have made assumptions about the carcinogenicity of the 16 herbicides that are inconsistent with the interpretation of other national organizations.

Statements in the DEIS on the very low exposure potential and evidence available regarding the very low toxicity of the materials of concern should be sufficient documentation to limit further concern about public exposure (sample inserts for text were given).

Response

The FEIS has been revised to more succinctly address pertinent environmental and human health impacts. In addition, a separate summary on "Characterization and Management of Risk" has been developed and is part of the FEIS.

We agree that the risk analysis has been designed to overestimate rather than underestimate potential risk to human health. This conservatism is an accepted scientific practice for dealing with uncertainties. We have added an example to the FEIS that shows what effect the cumulative overestimates of exposure have on the probability of toxic effects occurring.

We understand that our interpretation of cancer data uses worst case assumptions. We believe, however, that this method provides a consistent method of characterizing the risks to the public. We have added data from other nationally recognized scientific organizations where appropriate.

We added information on the probability of exposure to indicate the level of risk. However, we have provided information on the risk to the public based on comparing acute exposures to chronic toxicity levels.

Although the circumstances studied may be different, information on toxicity and hazards from the principle emissions of burning vegetation is not as incomplete or unavailable as stated in Chapter IV.

The incorporation of appropriate controls can minimize adverse human health effects and lower the probability of significant adverse environmental impacts.

Risk assessment and planning did not incorporate considerations of relative herbicide use patterns.

The importance of safety in reducing worker exposure and the risk is greatly understated in the DEIS.

There is a lot of data available on human health impacts from smoke (especially smoke from wood stoves) and other components of smoke. We are broadening our discussion of smoke and discussing applicability of smoke studies on other than slash burns.

Mitigation measures will substantially reduce the risks quantified in Appendix D. We have spelled this out more clearly in the FEIS.

Use patterns will be more fully discussed in the FEIS, particularly in the appendix on characterization and management of human health risks.

We have added additional information on reducing worker exposure through sound health and safety practices.

Richard Wilson
(Harvard University)

Comment

The definition of carcinogenic is not made and only implied. On pages 17 et seq., various phrases are used. There is no absolute definition of a nononcogenic chemical. One can merely say that it has not been found to be oncogenic (carcinogenic) in any study so far. In the discussion of carcinogenic potency (I would prefer this to "cancer potency" on p. 24), the point is blurred. Also on p. 24, the wording for simazine and tebuthiuron could be improved. Instead of "Available evidence does not indicate that tebuthiuron is carcinogenic," a better phrasing would be "tebuthiuron has not been found to be carcinogenic in any study so far." This is important, because it is later claimed that the cancer risk estimates are a worst case study. They are not. It is important to emphasize the respects in which they are not at the appropriate places. Under "carcinogenic (or cancer) potency" on p. 23, I would start with a disclaimer or else calculate an upper 95th percentile for carcinogenic potency for all chemicals, including those for which the potency value is not

Response

A more clearly defined discussion of oncogenic potential has been added to this section. To suggest that all of the chemicals used in this analysis are potentially carcinogenic would be inappropriate based on available study data. Uncertainties are clearly emphasized in this analysis.

statistically significant (therefore declared not to be carcinogenic by EPA) because the chemical could be carcinogenic, and the potency this high, without it having been detected.

Figure 3-2 is bad and misleading. As drawn, it implies linear axes, yet if so, human doses are much lower than drawn and the data more spread out. There is also a point at zero dose (background cancers).

The formula $-\ln(1 - P(d)) = a + (b \times d)$ is not a suitable formula for finding the carcinogenic potency b . It is unnecessary and misleading to use the phrase "one-hit model," particularly here and also anywhere in the EIS. The use implies that there is more biological meaning to the formula than is the case.

The figure has been revised to more correctly indicate the relative magnitude of potential human doses. However, this figure is intended to be a generalized illustration of the relationship between dose and carcinogenic potential. An expanded discussion of this illustration has been added in the FEIS. The carcinogenic potential of each chemical is discussed individually.

The discussion of carcinogenic potential for all of the chemicals and some of the calculations have been revised. With the exception of the cancer potency value for atrazine, all other cancer potency values have been derived using a multistage model. The cancer potency value for atrazine was derived in USDA (1984) using the one-hit model and laboratory study data. Therefore, these calculations are no longer presented in the EIS. The term "one-hit model" is conventional, even though the actual biological mechanism is not clear.

The definition of potency on p. 23 is good. This definition immediately leads to the next point, which is not stated but should be. "Thus potency is, in general, a function of the dose. It must be emphasized that the potency is derived here from data at high dose levels, and is only applicable to low doses by assuming the applicability of the formula." This would break two important concepts of sources of uncertainties that are confused in this EIS, the animal/man comparison and the high/low dose extrapolations.

It is not stated on pages 25-27, but I assume that the quoted potencies are potencies calculated for the animals mentioned. Not the human potencies derived therefrom.

Although it is common to express doses as mg/kg/day, it is formally incorrect unless parentheses are applied.

mg/kg/day means $\text{mg}/(\text{kg}/\text{day})$
= mg day/kg.

It is better as $(\text{mg}/\text{kg})/\text{day}$ or mg/kg day.

Explicit rewrite of pages 23-25. Section 3 appendix.

This addition has been incorporated into the discussion of cancer potency.

These concerns have been addressed in the revised version of Section 3 which clearly differentiates between cancer potency values derived for animals and humans.

Because the common expression in scientific literature of this dosage unit is mg/kg/day, the units are presented in this manner for consistency with similar documents. The meaning of the unit has been clarified in the document.

Thank you for the rewrite of the carcinogenic potency discussion in Section 3. Much of this information is no longer appropriate in the revised version of this section, which presents cancer potency values derived using the multistage model. Appropriate information was incorporated in the revised Section 3.

Section 5--Cancer Risk. Again, avoid the phrase "one-hit model." For example, p. 22, first line of Section 4. P.21 lines 24, 26. I prefer the use of the word formula rather than model to emphasize that the biological basis is slim. Explicit rewrite of page 21 in Section 5 (threshold versus nonthreshold approach to carcinogenic assessment).

Page 23, Section 5. Add: "For the cases where there are only two doses in the animal bioassay or there is no upward slope in the dose-response relationship, this computer program reduces to the simple, linear, no-threshold, model described in Section 3 and drawn in figure 3-2."

Several of these assumptions in the risk analysis give "conservatism factors" that multiply and cannot be distinguished, suggesting that risk might be overestimated by a factor between 10 and 100. The best predictor of human risk comes from:

- (a) averaging the animal studies and taking the most sensitive
- (b) assuming the "best value" of potency not the upper 95th percentile
- (c) assuming that carcinogenic potency in humans is equal to that in animals when doses are the same on a mg/kg day basis. If we ascribe all the differences to carcinogenic potency we can state the uncertainty about this statement.

Again, the revised version of the EIS does not use the "one-hit model" to determine cancer potency; therefore, the majority of this comment is no longer applicable. The revised EIS has been expanded to incorporate appropriate portions of this rewrite.

Thank you for the additional information. The revised edition of Section 5 has been expanded to include this information.

We acknowledge that many of these assumptions contribute to the overestimation of risk; however, these assumptions are consistent with the conservative approach of this analysis.

I/A Public Participation and Consultation

The bottom of p. 24 and top of p. 25 needs modification and amplification. (Examples provided.)

Page 28--It must be emphasized that these risks are "best estimate" risks, and for cancer risks "best estimate" risks in the framework of the linear no-threshold formula. The conservatisms of this section were not present and in particular K was chosen equal to 1. Thus the risks in the table are not strictly comparable to those calculated in the rest of the EIS. To make the cancer risks comparable, the risks in the table should be multiplied by about 20, and the amount to accumulate a one in a million risk of death divided by the same factor of 20.

Change Table 5-15 after:

"6 pounds of peanut butter
(aflatoxin B1)
1800 pints of milk (c)
(aflatoxin G)

c) modified from the reference to take account of the less toxic aflatoxin that is present in milk.

Many authors, Peto (1983) and Ames et al. (1987) believe that the twofold uncertainties in interspecies comparison and high/low dose extrapolation are so great that describing the cancer risk quantitatively in this way is misleading.

Thank you for these modification suggestions. The multistage model was used to calculate the majority of cancer potency values; therefore, this equation is no longer applicable.

This is a well-founded comment. A footnote and explanation will be added to the table and text, respectively.

The table was modified to incorporate these corrections.

We acknowledge that the extrapolation (interspecies and high/low dose) may tend to overestimate risk. Again, the quantitative analysis of cancer risk incorporated many conservative assumptions; however, these assumptions are consistent with the conservative approach of the EIS.

Wilson

Beginning of Section 5, "Human Health Risk Analysis," I find that there is not a proper contrast made between the approach for acute effects (margin-of-safety approach) and for chronic (cancer) effects where a risk is estimated. I suggest additions and changes to pages 1 and 2 to accomplish this. (Examples given.)

This section has been revised for clarity. The NOEL does not occur for a single exposure. The exposure used in the margin-of-safety calculation is a single dose. It is compared to a NOEL established using laboratory study data from chronic exposure.

Wallace D. Winters, M.D., Ph.D.
(University of California, Davis)

Comment

In Appendix D, section 3 page 2-last paragraph, toxicity is a progression of signs and symptoms that increase with time and dose, from subthreshold effects to lethality. Death is the easiest response to determine because it is an all-or-none response, whereas lesser toxicities are not.

On page 3, paragraphs 3 and 4, ppm in the animal diet is the concentration, but not the dose intake into the animal. Must take the daily dose intake per animal and base dosage on this plus the concentration in the food.

Line 7 makes no sense. No one sets these dose levels. They reflect the range at which the LD₅₀ is observed. They may be lower in chronic studies, but that is because the dosage is chosen that will not cause acute death; therefore, the dosage must be lower in a chronic study.

The last column of Table 3-1 should be presented in both metric and English equivalents and based on a 70-kg human.

Response

This paragraph has been revised in the FEIS to better define toxicity.

The discussions of routes of administration and dosing levels have been revised in the FEIS.

These discussions have been revised as appropriate to address these concerns in the FEIS.

For consistency with the other portions of this document, as well as other documents of this type, all measurements are expressed in metric units.

The upper graph of figure 3-1, is inaccurate and misleading. This is a dose response curve; therefore, the Y axis cannot be used for both number of animals that died (LD₅₀) and a progression of toxic symptoms. Some toxic, moderate toxic effects, etc., are a continuum of increasingly more toxic symptoms that are dose related.

Page 6, 2nd sentence-- "...dosed during the various stages of gestation." The middle period of gestation is the most commonly examined stage because since this is when fetal organs are often most sensitive to chemicals.

A more descriptive explanation of this graph has been added in the text to eliminate any misinterpretation.

You are correct in that many teratology dosing regimens do administer test material only in the middle portion of gestation; however, there are also many studies in which the test material is administered throughout the gestation period.

Appendix I/B

SECTION I

Selected Response Letters

This section contains letters we received in response to the Draft Environmental Impact Statement. The volume of letters would not allow us to print all of them. All letters were read, coded, and entered into our data base. These letters were selected to publish because they represent the full spectrum of thought and opinion which people forwarded to us during the public review and comment period.

NEPA requires publication of responses from government agencies and officials. Those letters follow the section of individual letters.

List of Individual Letters

- 01138 Anonymous
- 01468 American Fisheries Society, Oregon Chapter
P O Box 722, Corvallis, OR 97339
Steve Smith, President; Dave Buchanan, Chair Water Resources
Committee
- 011410 Audubon Society of Portland
35151 Northwest Cornell Road
Portland, OR 97210
Christie Galen, Conservation Committee
- 01309 Ms. Jo Broadwell
Route 1, Box 76
Halfway, OR 97834
- 01420 Don and June Carlton
RFD Box 55
Deadwood, OR 97430
- 01300 CIBA-GEIGY Corporation
PO Box 18300
Greensboro, NC 27419
Thomas J. Parshley, Regulatory Specialist
- 01004 Citizens for Environmental Quality (CEQ); Idaho Natural Resources
Legal Foundation (NRLF); and Georgia E. Hoglund, Authorized
Representative for CEQ and INRLF
4002 NE Cooper Road
Camas, WA 98607
- 01123 Douglas Timber Operators, Inc.
Suite 208, 3000 Stewart Parkway
Roseburg, OR 97470
A. Troy Reinhart, Executive Director
- 00640 The Dow Chemical Company
Midland, Michigan 48674
Richard J. Nolan, Research Leader, Biotransformation Group
Colin N. Park and Joanne Betso, Health and Environmental Sciences

- 01387 Du Pont
Walkers Mill, Barley Mill Plaza
Wilmington, DE 19898
Frederick O. O'Neal, Registration Toxicologist
- 01313 Earth First! Siskiyou
Box 212
Williams, OR 97544
Robert Brothers
- 00238 Robert E. Emmons
1064 West 5th Avenue
Eugene, OR 97402
- 01165 Stephen W. Hager
Cougar Creek
Deadwood, OR 97430
- 01312 Headwaters
PO Box 1075
Grants Pass, OR 97526
Julie Kay Norman, Coordinating Council
- 00407 I.B. (Ben) Iverson, Civil Engineer
30211 SE 40th Street
Fall City, WA 98024
- 00418 D.R. Johnson Lumber Company
PO Box 66
Riddle, OR 97469
D.R. Johnson, President and Manager/Ronald S. Yokum,
Corporate Counsel
- 00415 Lakeview Water Users, Inc.
HC 60 - Box 2980
Lakeview, OR 97630
Herb Jessen, Manager

- 01051 Lilly Research Laboratories
PO Box 708
Greenfield, IN 46140
David S. Negilski, Toxicology Project Leader
- 04962 Longview Chapter Society of American Foresters
Randall Greggs, Chairman
c/o International Paper Company
P O Box 579
Longview, WA 98632
- 01305 Paul E. Merrell and Carol Van Strum
7493 East Five Rivers Road
Tidewater, OR 97390
- 01304 Wendell R. Mullison, Herbicide Consultant
1412 N. Parkway
Midland, MI 48640
- 01484 Monsanto
800 N. Lindbergh Blvd.
St. Louis, MO 63167
- 011308 National Coalition Against the Misuse of Pesticides
530 7th Street, SE
Washington, DC 20003
Kevin Thorpe, Staff Entomologist
- 01409 National Wildlife Federation
Suite 606 Dekum Building
519 SW Third Avenue
Portland, OR 97204
- 01013 Northwest Coalition for Alternatives to Pesticides
P O Box 1393
Eugene, OR 97440
Norma Grier, Director, and Mary H. O'Brien
- 01386 Northwest Forestry Association
225 SW Broadway, Room 400
Portland, OR 97205
James C. Geisinger, President

- 01246 Oregon Environmental Council
2637 SW Water Avenue
Portland, OR 97201
John A. Charles, Executive Director
- 01302 Oregonians for Food and Shelter
567 Union Street, NE
Salem, OR 97301
Terry L. Witt, Executive Director
- 00702 Debbie L. Pickering
2499 N. North Bank Road
Otis, OR 97368
- 01470 Residents of Oregon Against Deadly Sprays
P O Box 1101
Eugene, OR 97440
Jan Wroncy, Spokesperson
- 00750 Roseburg Area Chamber of Commerce
P O Box 1026
Roseburg, OR 97470
Neal Walker, President
- 01126 Roseburg Resources Co.
P O Box 108
Roseburg, OR 97470
Leonard Gondek, Chief Forester
- 01192 William S. Seaman, Forestry Specialist
DuPont
Walker's Mill, Barley Mill Plaza
Wilmington, DE 19898
- 01084 Sierra Club, Oregon Chapter
2506 NE Halsey
Portland, OR 97232
Carol Lieberman, Chair/Teresa Carp, Pesticides Coordinator

- 01027 Society of American Foresters, Oregon
Oregon State University, College of Forestry
Corvallis, OR 97331
- 01311 Southern Oregon Northwest Coalition for Alternatives to Pesticides
P O Box 402
Grants Pass, OR 97526
Louise Racataian, SONCAP Volunteer Coordinator
- 01436 Southern Oregon Timber Industries Association
2680 North Pacific Highway
Medford, OR 97501
Roxi K. Smith, Staff Forester
- 00850 E.M. Sterling
1213 E. Shelby #6
Seattle, WA 98102
- 00876 Sunny Thompson
Star Route
Ashfrod, WA 98304
- 01010 Trout Unlimited of Oregon
P O Box 6225
Bend, OR 97708
Eric E. Schulz, Chairman Oregon Council
- 01415 Kindler Stout
130 Orange
Ashland, OR 97520
- 00908 Frank Vaughn
936 N. 7th
Lakeview, OR 97630
- 01434 Washington Forest Protection Association
711 Capitol Way, Evergreen Plaza Bg., Suite 608
Olympia, WA 98501
Stewart Bledsoe, Executive Director

- 01315 Washington Friends of Farms and Forests
P O Box 7644
Olympia, WA 98507-7644
Duncan C. Wurm, Executive Director
- 01307 Washington Wilderness Coalition
P O Box 45187
Seattle, WA 98145-0187
Kristen Shepherd, Administrative Assistant
- 01453 Western Forest Industries Association
1500 SW Taylor Street
Portland, OR 97205
Ralph Saperstein, Vice President, Forest Policy
- 01306 Western Washington Toxics Coalition
4516 University Way NE, Suite #6
Seattle, WA 98105
Karen Murphy, Director
- 01006 Western Wood Products Association
Yeon Building, 522 SW Fifth Avenue
Portland, OR 97204-2122
R.M. Fredsall, Director Resources and Environment
- 01444 Weyerhaeuser
Tacoma, WA 98477
J.P. McMahon, Vice President, Timberlands
- 01061 The Wildlife Society, Oregon Chapter
Oregon State University
Department of Fisheries and Wildlife, Nash 104
Corvallis, OR 97331-3803
John A. Crawford, President

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Response Form

To be most helpful, we need your concise and thoughtful comments on:

- ... The alternatives; telling us which ones you support, or changes that could be made.
- ... Our scientific information and analysis.
- ... The measures we have proposed to protect the environment.

~~This is an individual response and does not reflect agency opinion.~~

~~First, I would like to question some of the premises the document was developed upon. I find it interesting to equate the known hazards of smoke inhalation with hazards from herbicides. Long term effects of herbicides to the applicator are largely unstudied, therefore an unknown. It is inappropriate to group together the fire and herbicide hazards. There has been no analysis of the long term environmental effects of various herbicides upon the forested areas and the watersheds they occupy. Reduction of natural vegetation will interfere in the development of the forest, how? nutrient depletion? removal of valuable components of the interacting ecosystem (both plant and animal)?~~

~~The Hazards analysis also indicates 'estimated injuries'. Again, I question lumping together herbicide and fire related hazards. Becoming sterile as a result of herbicide exposure is hardly comparable to twisting an ankle while traveling over rough terrain during a prescribed burn.~~

~~Biological methods: the damage done by grazing animals is significantly different than the impact of insects used for vegetation control. How do I identify what percentage of the area managed by 'Biological methods' will be by grazing animals which impact the soils and watershed negatively?~~

~~Lastly, when someone mentions 'a decrease in habitat for wildlife species', I wonder if they are referring to black tailed deer and elk or Spotted Owls. It makes a difference.~~

~~Ideally, I would select 'C', but I am somewhat of a realist. 'A' is not acceptable, too much burning. 'B' is unacceptable because it gives too much license to interpret the plan to the bias of the land manager. I like 'D', but it scares me that 'Herbicides are available under this alternative, but will be the last option considered.'; there is no definition here, too much leeway. 'E' spells out what herbicides can be used and prevents aerial application; unfortunately this alternative chooses to manage more land and therefore will use more herbicides, fire, and mechanical methods. 'F' and 'G' are unacceptable because too many herbicides, mechanical, and fire methods by acre in the plan.~~

~~What would alternative 'D' be like with the restrictions on herbicide use that are in 'E'? That would be my preferred alternative. I would also include strong language of preference for insect control over grazing in 'Biological methods'. Strong support for preventative control by manual means over mechanical methods is preferred. Additionally, 'last options' must be documented and circulated to those outside the agency for comment. (It's much too easy to say 'oh, that won't work, because....' and then proceed to follow your bias.)~~

001468

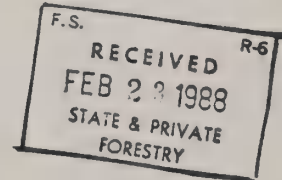
American Fisheries Society

Oregon Chapter
P.O. Box 722
Corvallis, Oregon 97339



22 February 1988

Mr Gary L. Larsen
Pacific Northwest Region
319 SW Pine, Box 3623
Portland, Oregon 97208



Dear Mr. Larsen:

The Oregon Chapter of the American Fisheries Society is pleased to comment on the Draft Environmental Impact Statement (DEIS), "Managing Competing and Unwanted Vegetation."

The Oregon Chapter is made up of more than 350 professional fisheries scientists, culturists and biologists who are dedicated to the wise management of fisheries resources and the advancement of fisheries science.

We support Alternative D of the DEIS; however, we have concerns for the maintenance and enhancement of the fisheries resource and would like to make suggestions to further these goals.

HERBICIDES

1. The herbicide option should not be used in watersheds supporting fish hatcheries.
2. We prefer backpack spraying to aerial spraying whenever possible.
3. Springs and seeps within the spray boundary should be identified and protected from contamination of herbicides.
4. We support the dimensions for buffer zones in the DEIS on flat and gently sloping lands; however, these distances must be increased on steeply sloped lands to ensure that the chemical will not drift into the streams or other waters.
5. To help ensure that water is not contaminated during herbicide applications we believe there is a need for quality control procedures. We fully agree that water monitoring be conducted as described in the Region 6 Water Quality Monitoring Guide for Pesticide Detection (R6-WS-040-1980) as

directed in the DEIS. This should include ground observers of the spray operation along buffers to notify the applicator of buffer encroachment and document violations. Spray cards should be placed within the riparian zone to identify sites of drift or direct application. Water sampling should be performed during and after the project and chemical analysis of the samples carried out by a qualified laboratory.

6. A report of the monitoring results should be submitted to the USFS Regional Forester and the Oregon Department Of Forestry within 60 days from the end of the spray project.
7. Past documentation indicates that equipment failure and accidental spills are major sources of water contamination. For this reason we believe a high level of effort must be made to ensure that this risk is reduced.

BIOLOGICAL METHODS (GRAZING)

1. We are unaware of the need for increased livestock grazing to control unwanted vegetation east of the Cascade Mountains. We are concerned about protecting riparian areas from livestock grazing in this region.
2. We support grazing as an alternative to herbicides for vegetation control on the west side of the Cascades if the riparian zones are protected.
3. We may support livestock grazing if the DEIS includes measures--such as fencing the most critical areas and providing watering facilities or structures away from aquatic habitat--to prevent riparian damage. We refer you to our publication "The Best Management Practices for the Management and Protection of Western Riparian Stream Ecosystems," by the American Fisheries Society, Western Division, 1982.

RESEARCH

1. We recommend long-term research to determine the effects of herbicide applications on stream ecosystems. Laboratory studies do not adequately model the effects of herbicides on a complete biological system.

Region 6 professional staff have put in an extraordinary effort in the organization and writing of the DEIS. They are to be commended on their work.

We hope you seriously consider our comments. We are available to offer our assistance in further planning. Please keep us informed on the progress planning and decisions of the DEIS.

Sincerely,

Barry C. McPherson for Steve Smith

Steve Smith
President, Oregon Chapter AFS

David V. Buchanan

Dave Buchanan
Chairman of the Water Resources Committee, Oregon Chapter AFS

001410



AUDUBON SOCIETY OF PORTLAND

A Branch of National Audubon Society

PHONE 292-6855

5151 NORTHWEST CORNELL ROAD

PORTLAND, OREGON 97210

TO: Gary Larsen
Vegetation Management Group Leader
USDA Forest Service, Pacific Northwest Region
P.O. Box 3623
Portland, OR 97208

FROM: Christie Galen, Conservation Committee
Portland Audubon Society
5151 N.W. Cornell Rd.
Portland, Oregon 97210

SUBJECT: Managing Competing and Unwanted Vegetation DEIS

DATE: February 13, 1988

These comments constitute a response from the Conservation Committee of the Portland Audubon Society. The Portland Audubon Society, a 5000 member chapter of the National Audubon Society, shares its parent organization's long-standing concern about the impacts of pesticides and herbicides on our wildlife, native plants, and overall habitat base. Audubon greatly appreciates the public involvement you have allowed in creating this voluminous DEIS, Managing Competing and Unwanted Vegetation. We also appreciate your including an alternative that resembles integrated pest management.

Of the three "preferred" alternatives, Alternative D comes the closest to meeting our approval. It emphasizes prevention of vegetation problems, allows treatment only when there is evidence that significant damage or growth loss will occur and selects methods that will minimize adverse environmental effects. It also allows the use of herbicides only as a last resort. This is a vital concept, but how is last resort to be determined? Is the process spelled out for managers to ensure that all other vegetation management methods are implemented first? Which herbicides will be used?

Alternative E eliminates the use of 2,4-D, amitrole, fosamine and diuron because of health concerns. These health concerns should be sufficient evidence to eliminate these herbicides from any alternative and they should never be used. When a herbicide is used as a last resort, the least toxic herbicide should be chosen; one that will not persist in the soil, one that will not leach through the soil and contaminate ground water on the site, and one that will not harm wildlife and non-target plants. Further, herbicide formulations contain secret and inert ingredients. Who assesses the toxicity of these ingredients? How can the public assess them if we don't even know what they are?

Is there a "healthy" herbicide that will have no adverse environmental effects? Research tests for environmental health effects are inadequate. Data that have been gathered on a limited number of species cannot demonstrate the effects of herbicides on the entire forest ecosystem. It is necessary to consider and research the effects of herbicides on entire food chains in the forest

from miniscule soil and aquatic micro-organisms to the large vertebrates. How long will it take research to discover these potential toxic effects? It took 10 years of research to determine that peregrine falcons were disappearing from California because the DDT which accumulated in their bodies had caused eggshell thinning which caused reproductive failure. There are not enough data on the effects of herbicide spraying on forest ecosystems to conclude that herbicides pose no significant threat to them.

Within the forest, riparian zones and wetlands are extremely important to wildlife and fish populations. The DEIS mentions buffer strips of 100' and 200' as a means to protect riparian zones and wetlands from herbicide spray drift. What is the justification for this minimal boundary. It is not enough to protect ground and surface water from contamination that will potentially harm the wildlife that concentrate in these areas.

Each forest needs site specific vegetation management guidelines adapted to their forest needs and the specific problematic species of their forests. They need to have guidelines that spell out clearly the meaning of "last resort." If a herbicide must be used it must be chosen selectively to minimize environmental and health damage. We endorse Alternative D but only if herbicides are used intelligently and truly as a last resort.

001309

Gary Larsen
USFS Region 6
POB 3623
Portland, Oregon 97208

Dear Mr. Larsen,

I support Alternative C from the Draft Environmental Impact Statement on Vegetation Management for Oregon and Washington. Risks to human health, the environment, and the wood products industry are significant should applications of herbicides and pesticides to "unwanted vegetation" be the preferred alternative to other options. Uneven age management with rotation cycles of 200 to 400 years provides timber quality harvest, nourishes plants and wildlife, stabilizes hillsides, provides water and serves the spiritual and recreational needs of human beings.

In the controVersy over herbicides, the greatest irony of all is that these chemicals, which are sprayed to speed the growth of commercial timber, may actually be damaging and destroying the very forests they are intended to help. When unwanted brush, trees, and other vegetation are removed, they can no longer play their vital role of providing nitrogen for the soil and habitat for small mammals, insects, and fungi which are necessary for the maintenance of a healthy forest ecosystem. The destruction of "weeds" as natural fertilizing agents has forced lumber firms to drop nitrogen-rich fertilizer into such areas. In addition to the added expense, a 1961 study showed that 2,4,5-T and 2,4-D slowed the growth of conifers after being applied. Another study showed that more than a third of the crop was destroyed after brush that had been killed off by herbicides broke off under the weight of snow and crushed the young trees.¹

Dr. Jan Newton, a highly respected economist and research consultant who has had contracts with the USDA and HEW (now HHS), concluded in 1980 that "not a single study could be found demonstrating increased growth of trees because of herbicide application."

She has also stated the USFS and Oregon Department of Forestry at OSU "purposefully misled the public concerning the employment impacts of herbicide use." She also found that the cost of manual labor eradicating unwanted vegetation may often be comparable to the cost of spraying.² Please consider these findings when analyzing 'Change of Jobs', and 'Present Net Value'!

I urge you to consider Chris Maser's research on Old Growth, and especially his publication of The Seen and Unseen World of the Fallen Tree.³ He feels that unless we turn to longer rotation cycles and uneven age management, the quality and quantity of our timber and forested lands will be severely threatened. If such practices were allowed on a long term basis, the cost-ratio on quality timber harvest accelerates, wildlife habitat is maintained, the cycling of nutrients and water sustains the forest eco-system, and possibly the need for chemical eradication of "unwanted vegetation" disappears.

A hundred years from now, will our trees require, rather than simply prefer, the application of fertilizers, pesticides and herbicides? And, if so, will we be in a position to support their addiction? Can we afford to commit and change the biological diversity and inter-dependence of our forests to a perpetual dependency on artificial supports? NO! The HEALTH AND VIGOR the Forest Service is mandated to maintain within the forest ecosystem is destroyed if herbicides and pesticides interrupt natural life cycles.

I am also concerned about the global warming trend, and how that might affect usual dosage of herbicides and pesticides within a given region. Could you please look into this, and adjust the amount and perhaps time of the option of spraying? From the October Acreage it stated that reservoirs in Eastern Oregon are 200% below normal and that there is an 8% chance of reaching normal water levels. This may affect the toxicity of any chemical and may seriously afflict humans, animals, birds, fish, plants.... If there is any question at all as to the harmful side affects, I do not feel we should use any herbicide until we have sufficient studies and thorough examination of each area.

I would like to see prepared a full forest-wide "reforestation EIS. for each National Forest. This study should incorporate information learned from a full monitoring of all existing plantations in

all of the various vegetation types unique to each Forest and not described in any existing document. Within the context of known and well-evaluated past practices, it will be possible to look at the consequences of alternative methods of timber harvest, reforestation, release, and thinning.

I would like to see Toxic-Free Zones surrounding salmon spawning beds, important wildlife habitat, and all residences. Also, an addition to the implementation guidelines which shall ensure that individuals will be exposed to toxic chemicals only with their "informed consent". I do not support aerial spraying because of the dangers in drift and complete massive waste as well as the imposing danger on non-target life forms.

As to the use of 2,4-D, it has been demonstrated over and over in laboratory conditions, personal experience, and even on its label the extreme toxicity of this chemical⁴. I am strictly against the spraying of 2,4-D, a member of the phenoxy herbicides. . Of the 75 different types of compounds called Dioxin, TCDD has caused death cancer, birth defects and other disorders in lab animals-in such small doses as to ^{be}inconceivable; such as parts per trillion. The "unknown" inert (Dioxin, Formaldehyde, Benzene and Xylene) should not be used until we have verifiable proof they are "known safe".

The general lable prohibition against contamination of water in the use of 2,4-D contradicts its usual routine use in water. It may occur directly via an overspray or indirectly as a result of fallout from the air, runoff from the land after rainfall, leaching into groundwaters from the land, or carriage in irrigation return flows. In one report 2,4-Dcontamination of groundwaters occurred five miles from the source and fourteen years after initial dumping was documented.⁵

Included, on page 4 and 5 is a list of Human Health Effects of 2,4-D. In addition is a copy of Ruth Shearer's report on this chemical. Both justify the dangers and NO USE status of 2,4-D,

U/S JO BROADWELL
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February 15, 1988

001420

RFD Box 55
Deadwood, Oregon 97430
11 February 1988

Vegetation Management Group Leader
USDA Forest Service
Pacific Northwest Region
P. O. Box 3623
Portland, Oregon 97208

ATTN: Mr. Gary Larsen

Gentlemen:

Enclosed is our response to your request for public comments on the Draft EIS for Managing Competing and Unwanted Vegetation.

Forest Service policy is of great interest to us, both because our farm is surrounded by Forest land and because of a deep and long-standing respect and affection for the National Forests of our nation.

We would like to express our appreciation to the members of the IDT and other Forest Service people who have been generous with their time and professional wisdom to aid our understanding of this important and complex aspect of forest management. Thank you all very much!

Sincerely,

Don + June Carlton
Don and June Carlton
503-964-5571

RESPONSE TO: MANAGING COMPETING AND UNWANTED VEGETATION
 Draft Environmental Impact Statement
 Region 6 11 February 1988

OVERVIEW

The general theme of this response is predicated on a view of Region 6 Forests as predominantly healthy productive natural systems richly endowed with timber and water resources. Those portions of the Forests managed for timber production are now in transition to systems dominated by human manipulation with the objective of increased productivity. This places heavy responsibility on Forest managers to insure that their actions avoid the adverse effects which often accompany man's intervention in biologic systems.

Forest management, including the management of unwanted vegetation, should be guided by two complementary principles:

1. Forest operational management should be conservative, Concern for long term health, productivity and diversity should dominate. Assumptions, estimates and predictions should be biased conservatively, proportionate to the uncertainty. Monitoring should assure early detection of adverse effects. Application of manipulative practices should be cautious, on a scale compatible with the confidence in knowledge.
2. Forest research and innovation should be broad based and aggressive. Disclosures in this DEIS and others clearly show significant deficiencies in the level of understanding of important aspects of forest management.

COMMENTS APPLICABLE TO ALL ALTERNATIVES

1. Consideration for the management of unwanted vegetation should be given substantial weight in all relevant forest management decisions. Avoidance should be emphasized.

DISCUSSION: The most frequently used methods of vegetation management have adverse effects which cannot be wholly mitigated. We are particularly concerned with long-term effects. Vegetation management should not be just an unavoidable after effect of other forest decisions.

For example:

- a. Longer rotations reduce the frequency of vegetative management actions and reduce the overall effect of competing vegetation over the whole rotation. While long rotations do not fair well in PNV analysis they do in fact produce nearly as much timber, and of higher quality.

Page 2

b. Selection of harvest methods should be influenced by vegetation management considerations.

c. Decisions to harvest "marginal" (for timber) lands and analysis of below cost sales should include the adverse effects (especially long term) of consequential vegetation management actions.

d. Where a delay in reforestation will increase the need for vegetation management, Standards and Guidelines should include a requirement that seedlings will be available for timely restocking before the unit is cut. No seedlings - no harvest!

e. The lower quality and higher cost of thinned timber should be considered when a decision for action against competing vegetation to reduce seedling and sapling mortality is to be made. Final stand density should be the goal. In our Forest at least (Siuslaw) commercial thinning will be a below cost sale in many stands.

2. Active public participation in site-specific project planning implementation and monitoring should be encouraged, including the ability of the public to influence decisions.

3. The monitoring program should include a cumulative review of site-specific project plans on a Forest-by-Forest basis to assure that the theme and emphasis of the selected alternative is being accomplished.

DISCUSSION: Alternatives B, D, E and G differ primarily in matters of emphasis. It would be difficult to tell from any individual project which alternative was being followed.

4. Project monitoring should give particular attention to the identification and analysis of unplanned events having adverse effects. Preventive actions to preclude their recurrence should be incorporated into future project plans.

5. Regional Guidelines for project planning should direct managers to discount cost effectiveness by a factor of (?) when selecting vegetation management methods to give greater weight to minimizing human health risks and adverse effects on ecosystem structure and function.

DISCUSSION: The decision process for balancing priced and unpriced costs is inherently difficult. Although detailed guidelines are an evolving process, project managers should be given as clear direction as possible to assure consistent implementation. Guidelines also help the public understand and participate at the project level. We would tend to choose a discount factor of 5 or 10 but that decision is best left to professional forest managers.

6. The FEIS should include objective data on the achieved effectiveness of important mitigation measures.

Page 3

7. The monitoring program should provide sufficient data to assess the effectiveness of mitigation measures against objective standards on an on-going basis.

DISCUSSION: Mitigation measures, while helpful, are not always completely successful in practice. An objective measure of their effectiveness is needed to properly evaluate the probable combined effect of a vegetation management method and its associated mitigation actions. This information should be used in project level decisions and for periodic assessment of the adequacy of mitigation measures. Goal-oriented qualitative guidelines (e.g. II-73) are inadequate management tools in this application.

Of particular concern are mitigation measures for prescribed burning and use of herbicides which have a high risk of environmental damage. Prescribed burning does not always produce only light burns. The difference between a light burn and a severe burn is highly significant to soil and watershed conditions. There is only limited data to support the adequacy of mitigation measures for aerial application of herbicides to protect water resources. Insufficient reliable information is available to make measure #9 (II-83) an effective mitigation tool. Particularly with respect to persistence and mobility of some of the listed herbicides and their effect on soil and aquatic organisms.

8. The Forest Service must establish its own standards for protecting the National Forest environment, resources and long-term health and productivity.

DISCUSSION: Provisions of the Oregon Forest Practices Act, EPA standards and regulations and Oregon Best Management Practices are grossly inadequate in their present form and do not satisfy the intent of legislation governing National Forest policy.

COMMENTS APPLICABLE TO WATER RESOURCES

9. Vegetation management for water resource maintenance and enhancement, both quality and quantity, should be a co-equal program goal with timber and range resource enhancement.

DISCUSSION: A major portion of Pacific Northwest water originates within the forests of Region 6. As other watershed lands are developed National Forest lands will have increasingly greater significance as a water source. At this time we still enjoy excellent water compared with most parts of the country. There is no strong public advocacy for water as there is for timber and environmental issues and water is not a priced benefit. (It is unfortunate that we do not have the equivalent of LTSY for water resources.) Within the long time horizon which prudent forest management requires and consistent with the principle of multiple use, the Forest Service should give great weight to the importance which the next generation will place on an abundance of high quality water, at least equal with timber resources.

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The issue is broader than the 6 million acres now managed for domestic watershed. Nearly all forest lands are in fact watershed. The goal for water resource management should be the best possible rather than the least permissible.

While the effects of managing unwanted vegetation on water resources may be small in comparison with timber harvest they are NOT zero, or even insignificant. Many vegetation management decisions pose a conflict between water resources and timber resources, each should be given equal weight.

10. In site preparation planning, manual methods, even at higher cost, should be given preference over burning wherever practicable. Specific weight should be given to the cumulative adverse effects of burning (even light burns) within a watershed.

11. Slash treatment should include improving stream structure with large woody material wherever needed and practicable, particularly in first order streams.

12. When using prescribed burning, increase mitigation measures to protect areas adjacent to intermittent streams.

13. When using aerial application of herbicides, increase mitigation measures to protect areas adjacent to intermittent stream beds, whether flowing or not, to a distance of 100' minimum.

DISCUSSION: Intermittent streams are important components of the water systems of the forest. They are active during winter and spring, and frequently flow year around just below the surface. Their structure and the adjacent area have highly significant effects on watershed quality. If provided with adequate "steps" they absorb stream energy, slow and even storm flow and so lessen downstream flooding, stream scour, and damage to fish habitat. They can act as effective filters to retain valuable top soil and reduce turbidity. Intermittent streams, properly structured, are important sources and dispensing agents for nutrients, organic matter and gravel to supply the downstream food chain and fish habitat.

The information available on herbicide persistence, mobility, and toxic effects on soil and aquatic organisms is insufficient to assure adequate protection of water resources and therefore stringent mitigation measures are justified if herbicides are to be used at all. A 50' buffer does not provide enough operational margin.

COMMENTS APPLICABLE TO HERBICIDES

14. In any alternative, use herbicides only as a last option, and then only for lands of average or above productivity.

15. Define "last option" to mean: allow the use of herbicides ONLY if no other vegetation management tool will achieve an acceptable level of control.

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16. 2,4 D, Amitrole, Diuron and Fosamine should not be used.
17. Burning herbicide-treated vegetation should be prohibited.
18. Herbicides should be prohibited in private or municipal domestic watersheds.

DISCUSSION: We are troubled deeply by the introduction of biologically active synthetic chemicals into the environment. Both as a human health risk and for possible adverse effects on the health and productivity of the forest. It is clear from the disclosures in the DEIS that important information is unavailable, research is scattered and results often conflicting. Conclusions are frequently driven more by attitudes and interests than by the weight of the data available. Under these circumstances a cautious approach is required. Present regulations and short term economic incentives result in aggressive use of herbicides by private forest managers. Thus a portion of the nation's forests are already being put at risk. The Forest Service, as stewards of public forests for long-range, multiple use objectives should adopt a very conservative policy.

At the same time we recognize that, with the current state-of-the-art, herbicides are apparently the only effective vegetation management tool for certain conditions found in Region 6. The concept, introduced in Alternative D, of using herbicides only as a last option seems especially appropriate. The sites planned for herbicide treatment however should meet a more stringent requirement for productivity to justify the added risk. Mature stands on marginally productive land with the potential for serious regeneration problems without the use of herbicides should be reserved for use other than timber production, such as watershed.

At the operational level the concept of "last option" must be supported with clear, detailed unambiguous guidelines to insure consistent implementation. The FEIS should include a complete statement and discussion of these guidelines to help the public understand their intent and probable effect.

19. The Forest Service should conduct or sponsor its own research and development of herbicide formulations and procure herbicides to its own specifications. Particular attention should be given to product uniformity, quality control, impurities and "inert" ingredients.

DISCUSSION: Proprietary formulations are subject to a variety of changes over time which can add to the difficulty of evaluating the complex and highly variable effects of vegetation treatments. There is well founded concern for the human health effects and environmental effects of impurities (side reaction products) and "inert" ingredients. With the current very limited knowledge it is only prudent to control the products as closely as possible.

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COMMENTS APPLICABLE TO SOIL RESOURCES

20. The importance of soil and forest litter decomposers (fungi, bacteria, actinomycetes, and invertebrates) should be highlighted by including them in their own section in Chapter 4 of the FEIS. The conclusions should clearly state that present data is fragmentary and inconclusive.

21. There is urgent need for comprehensive research on the effect of the manipulation of vegetation on forest decomposers and nutrient recycling.

DISCUSSION: More attention and concern needs to be given to the long-term productivity of forest soils. The roles of biomass and soil and litter organisms in nutrient recycling and biological control of pathogens are of paramount importance, yet our knowledge is fragmentary. Our study of some of the references on the effects of herbicides shows that the evidence stated in the discussion section is misleading and the conclusions drawn are unsubstantiated (IV 20-21).

For example:

1. The statement (IV-20), "Norris et al. (1987) measured a 50% loss of triclopyr in 3.7 days . . . Western Oregon" should be deleted or amended. This statement applies only to the GRASS sampled on the site. Triclopyr in top SOIL samples (0 to 15 cm) at the same site in the same experiment actually increased from 0.55 mg/kg (day 0) to 3.1 mg/kg (day 28). The half life of triclopyr was calculated to be 81 days. It is difficult to evaluate how much triclopyr from the grass was merely transferred to the soil, because the sample size is given in weight only, rather than surface area.
 2. The first paragraph (IV-21), "Many soil organisms can metabolize herbicides . . . stimulation of certain populations" is too general. Include a column on Table IV-10 (IV 26-27) showing which herbicide is metabolized by soil organisms and its effects on the decomposers.
 3. Considering the conclusions, (a) it is unclear how any of the mitigation measures (II 81-84) deal with soil and litter decomposers except ones in riparian areas, (b) the data in the discussion section does not support the conclusions that "90% or more of the herbicide used will be intercepted by foliage and thus will not reach the soil until it is mostly decomposed" (IV-21).
22. Fire management site evaluation and implementation should stress the desirability of retaining biomass on site for biologic decomposition and nutrient cycling in preference to utilization and prescribed burning.

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DISCUSSION: There are studies which suggest that current forest practices (prescribed burn, YUM) leave insufficient biomass capital to maintain long-term soil productivity.

It seems clear that there is insufficient data to fully appraise the effects of the use of herbicides and prescribed burning on soil productivity and organisms and therefore vegetation management methods should be selected which interfere least with natural processes until sufficient research has been done to provide conclusive answers.

PREFERRED ALTERNATIVE

23. Alternative D should be chosen as the preferred alternative for the FEIS.

DISCUSSION: Alternative D appears to emphasize an appropriate mix of conservative action and creative innovation, and, if properly implemented, would be most responsive to the issues of greatest concern to us. With the District Court ban on the use of herbicides a number of new approaches to vegetation management have been tried and proved effective. Continued progress seems probable if the incentives remain.

The strong emphasis on public participation in site-specific projects is especially appealing. Our land is almost completely surrounded by Forest land and our domestic and irrigation water originate on Forest land. We are anxious to participate to the fullest extent possible in decisions which directly affect us. We also believe that broad public participation at the project level will materially improve public understanding and acceptance of necessary Forest activities and reduce social conflict within the community.

The sensitivity analysis conducted on the effect of the ASQ prediction on PNV ranking of the alternatives was particularly helpful. It calls attention to the small return to be gained by investing in additional vegetation management on marginally productive land (Alt. G). It shows alternative D to have a sound mix of costs and benefits and the potential for very good economic performance with the expected improvement in effectiveness over time.

We endorse limiting the use of herbicides to a "last option", but we question that the prediction of treating 27 thousand acres annually really represents "last option" use.

The decision of the preferred alternative for the FEIS should be based on grounds other than economic, considering that the costs of vegetation management are a small part of overall forest costs and that the predicted changes in economic effects are small (except Alt. C) and highly uncertain.

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The DEIS evaluation (II-22) shows that D is the only alternative predicted to increase long term soil productivity, to have the least risk to human health (except Alt. C) and despite fewer acres being treated, to have an effectiveness near current levels.

We question the projected increase in future wildfires (5th decade) for Alternative D. Was allowance made for expected improvements over 50 years in wildfire detection and suppression and manual and mechanical fuel reduction methods? Was allowance made for reduced exposure to wildfires due to the smaller number of prescribed burns in Alternative D?

FINAL COMMENT

However carefully drafted and conscientiously reviewed, the ultimate value of the FEIS depends exclusively on effective implementation and monitoring. Clearly there are huge gaps in knowledge essential to sound project level decisions. It is incumbent on Forest management to provide direction and resources for research, monitoring and analysis to fill these gaps with objective data and to ensure its dissemination to project decision makers. The effectiveness of the five step process is dependent on sound judgment based on reliable information.

While perhaps beyond the legitimate scope of these comments we are compelled to call attention to the urgent need to provide adequate professional staff levels to realize fully the benefits of the five step process. Just as this EIS expands the horizon for responsible vegetation management, its implementation will expand the management activities necessary for its fulfillment.

Don and June Carlton

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CIBA-GEIGY

001600

February 12, 1988

Gary Larsen
Vegetation Management Group Leader
USDA Forest Service, Pacific Northwest Region
P.O. Box 3623
Portland, OR 97208

Dear Dr. Larsen:

Attached to this letter are comments which are being provided by the Agricultural Division of CIBA-GEIGY Corporation in response to the Draft Environmental Impact Statement, "Managing Competing and Unwanted Vegetation.", Request For Participation #6. CIBA-GEIGY is pleased to be able to assist the Forest Service in the development of this document and hopes that our comments will be useful in developing the final EIS.

The attached comments focus exclusively on the toxicology and risk assessment for atrazine, a very important product for CIBA-GEIGY, because it is here that the need for more accurate information is the greatest. However, generally speaking, it appears that the information cited within the EIS for environmental effects and fate of atrazine accounts for only a fraction of the information available in these areas. This chemical has an extensive data base which has been developed by CIBA-GEIGY in support of EPA registration, not only in the health effects area, but also in the ecological and environmental effects areas. Generally, the data presented within the EIS constitute literature references, which for the most part present negative data. CIBA-GEIGY encourages the Forest Service to evaluate all the available information on health and environmental effects of atrazine before finalizing this EIS. In particular, CIBA-GEIGY would be glad to provide summaries of environmental data we have generated over the years to the Forest Service for consideration, if requested. Summaries of the available toxicology data recently submitted to the EPA for the reregistration of atrazine are presented in the attached comments.

Page 2

Again, CIBA-GEIGY hopes the Forest Service will find our comments useful in preparing the final EIS and offers to share any environmental data results which are requested. If there are any questions concerning matters contained within this submission, please do not hesitate to contact us.

Sincerely,

Thomas J. Parshley
Regulatory Specialist

enclosures

cc: Paulette L. Pyle, Oregonians for Food and Shelter

SUMMARY

Toxicologic evaluation of atrazine during the recent reregistration process indicates atrazine is not mutagenic, teratogenic, reproductively toxic nor is it oncogenic in the mouse. Atrazine causes high-dose cardiotoxicity in dogs and it is a rat mammary tumorigen. Although atrazine has been implicated as a mammary tumorigen, evidence indicates that the effect may be hormonally mediated and, as a result, a threshold for the formation of mammary tumors may exist. In that case, a safety factor approach could be used to regulate exposure to atrazine, thereby greatly increasing its margin of safety. Moreover, more than thirty years of production and handling of the active ingredient shows no indication of adverse health effects in humans.

CIBA-GEIGY would like to take this opportunity to respond to toxicology information pertaining to the herbicide atrazine which has been included in the United States Department of Agriculture Forest Service (U.S.D.A.F.S.) Draft Environmental Impact Statement.

Toxicology Assessment

During the 30 years that atrazine has been manufactured, CIBA-GEIGY has conducted numerous acute, subacute, sub-chronic, chronic, teratology, reproductive toxicology and mutagenicity studies with the chemical. The results of these many studies have been submitted to the United States Environmental Protection Agency (EPA) as part of the registration and/or reregistration process for agricultural chemicals. Reports of the studies supplied to EPA were considered by CIBA-GEIGY to be proprietary information and as such have not been published in refereed journals.

However, because of the longevity and widespread use of the chemical, a number of independent researchers have performed a variety of toxicology or mutagenicity studies with atrazine and have published the results in various journals (some peer-reviewed, many not). In that context, studies accepted for publication are those that (generally) report "positive" findings. Because of that, there are reports in the literature of adverse toxicologic or mutagenic findings with atrazine. In many instances, the experimental design of the purportedly "positive" studies was incomplete or lacking in scientific validity (e.g., no control group, exaggerated doses, inappropriate route of exposure, etc.). In other instances, the results of some studies could not be duplicated by other scientists. Further, other studies were conducted in test systems with little applicability to human toxicology (e.g., plant microsomes, plant chromosomes, etc.).

Review of the U.S.D.A.F.S. document indicates that the Forest Service has access to data submitted by CIBA-GEIGY to EPA. CIBA-GEIGY requests that the Forest Service thoroughly review and summarize those data and include the results in a "weight of evidence" assessment of the toxicologic, mutagenic, and oncogenic potential of atrazine.

Additional toxicology studies (mouse oncogenicity, chronic dog, rat multigeneration) have been submitted to EPA apparently since the Forest Service review of the atrazine toxicology data base at EPA (see reference list at the end of this document). The results of those studies are summarized below. The results of the rat chronic study are also summarized. It should be noted that submission of those studies fulfills all "data gap" requirements for the

- 2 -

reregistration of atrazine. Also following is a toxicology profile of atrazine, a review of mutagenicity data as well as a "weight of evidence" assessment of the oncogenic potential of atrazine and CIBA-GEIGY's conclusion regarding appropriate risk assessment analysis.

Overall Atrazine Toxicology Profile

<u>Study Type</u>	<u>Experimental Results</u>
Acute Toxicity	
Oral LD ₅₀	1869 mg/kg
Dermal LD ₅₀	>3100 mg/kg
Inhal. LC ₅₀	< 0.7 mg/l
Skin & Eye Irritation	Negative
Skin Sensitization	Positive
Mutagenicity	Negative (Weight of Evidence)
Teratogenicity	Negative
Rat (NOEL)	10 mg/kg
Rabbit (NOEL)	1 mg/kg
Reproductive Tox.	Negative
Rat (NOEL)	150 ppm
Chronic Dog (NOEL)	150 ppm
Chronic Rat (NOEL)	Mammary tumors 10 ppm
Chronic Mouse (NOEL)	Negative for Oncogenicity 10 ppm

Results of Newly Submitted Atrazine Studies

Mouse Oncogenicity

Atrazine Technical was administered in the diets of four groups of male and female mice (60/sex/group) at concentrations of 10, 300, 1500, or 3000 ppm for at least 91 weeks. An additional group of mice (60/sex/group) received untreated rodent diet and served as controls. Ingestion of Atrazine

- 3 -

Technical at levels as high as 3000 ppm did not result in an increased incidence of neoplastic change. Effects were observed in this study and included: 1) decreased survival in high-level females; b) reductions in mean body weight and percent body weight gain at concentrations ≥ 300 ppm; and c) an increased incidence of atrial thrombi at concentrations ≥ 1500 ppm. Secondary effects included reductions in mean food consumption and water intake at concentrations ≥ 1500 ppm; reductions in mean hemoglobin, RBC count, and hematocrit at concentrations ≥ 1500 ppm; and alterations in several mean absolute and/or relative organ weight measurements at a concentration of 3000 ppm. The results of this study indicate that the no observable effect level in mice was at least 10 ppm, and that based on survival and body weight data, the maximum tolerated dose was exceeded at a dietary level of 3000 ppm.

Chronic Dog

Atrazine Technical was administered orally via the feed to 4 groups of dogs (4-6/sex/group) for at least 52 consecutive weeks at dietary concentrations of 0, 15, 150, or 1000 ppm. Standard clinical evaluations were performed on all dogs. Treatment-related effects occurred at the highest dietary level of 1000 ppm and included: 1) mortality in one dog; 2) clinical signs including cachexia and ascites; 3) slight reductions in body weight and percent body weight gain; 4) reductions in feed consumption; 5) physical examination findings including irregular heartbeats and increased heart rate; 6) moderate electrocardiographic alterations including increased heart rate, decreased PII values, atrial premature complexes, and atrial fibrillation; 7) slightly decreased erythroid parameters and increased platelet counts; 8) slightly decreased serum protein and albumin; 9) slight alterations in absolute heart and relative liver weights; 10) moderate to severe gross or microscopic lesions consisting primarily of dilatation of the right and left atria of the heart, and myocardial degeneration (atrophy, myolysis) of the atria. The cardiac findings were considered direct effects of Atrazine administration while many of the additional findings were considered secondary. These results indicate that the no-observable effect level in this study was 150 ppm and that, based on the cardiotoxicity observed, the maximum tolerated dose was exceeded at 1000 ppm.

Rat Multigeneration

Atrazine Technical was administered to two generations (F_0 , F_1) of male and female rats at dietary concentrations of 0, 10, 50 or 500 ppm. There were no compound-related mortalities, clinical observations, changes in reproductive parameters or perinatal or postnatal effects. During the pre-mating period, reduced food consumption was observed in F_0 and F_1 animals of both sexes at 500 ppm, as were reduced body weight and body weight gains. Body weights were also reduced for females of both the F_0 and F_1 generations during gestation and lactation. There were no remarkable gross or microscopic findings in any of the reproductive organs of the F_0 , F_1 or F_2 generations. Relative testes weights were increased in the 500 ppm F_0 and F_1 males as a result of reduced terminal body weights. Based on these results, it was concluded that Atrazine Technical does not cause any impairment in reproductive performance in rats fed a maximum tolerated concentration of 500 ppm for two successive generations. The no-observable effect level (NOEL) was considered to be at least 50 ppm.

Atrazine Chronic Rat Study

Atrazine was oncogenic in female CD-1 Sprague-Dawley rats. An increase in carcinomas of the mammary gland was observed in females fed 70, 500, or 1000 ppm atrazine for 2 years. There was also an increase in the incidence of fibroadenomas/adenomas (1000 ppm) as well as all mammary tumors in females receiving 500 and 1000 ppm when compared to controls. No oncogenic effects were observed at 10 ppm. There was a decrease in mean body weights of males and females receiving 500 and 1000 ppm. Survival was decreased in high-dose females but increased in high-dose males. Red cell parameters (hemoglobin, hematocrit, and red cell count) were decreased in high-dose females but not in males. The serum glucose level was decreased in high-dose females at 3, 6, and 12 months and serum triglyceride levels tended to be decreased in high-dose males throughout the study; however, the toxicologic importance of the clinical chemistry findings is unclear. There were decreases in organ-to-body weight ratios in high-dose animals, which were probably the result of body weight decreases. Hyperplastic changes in high-dose males (mammary gland, bladder, and prostate) and females (myeloid tissue of bone marrow and transitional epithelium of the kidney) were of questionable toxicologic importance. There was an increase in retinal degeneration and in centrilobular necrosis of the liver in high-dose females and an increase in degeneration of the rectus femoris muscle in high-dose males and females when compared to controls. Based on decreased body weight gain, the LOEL for chronic toxicity in males and females is 500 ppm and the NOEL is 70 ppm.

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Mutagenicity Review

A recent review of the existing mutagenicity data (literature references and CIBA-GEIGY mutagenicity studies) has been completed by Dr. D. Brusick, an acknowledged expert in the field of genetic toxicology (a copy is attached).

The objectives of his evaluation were to (1) reconcile the inconsistencies of test results within the data base, (2) interpret the significance of the possible different bio-activation of atrazine in plants and animals, and (3) draw a conclusion regarding the possible genetic toxicity of atrazine to humans.

It is not scientifically possible, nor totally necessary to reconcile all the test-response conflicts for atrazine. In some cases such as the *Drosophila* sex-linked recessive lethal assays it is relatively easy to dismiss the studies in which sample sizes varied and nonconcurrent negative controls were used. In other cases, such as the mouse dominant lethal studies, it appears that two studies using approximately equivalent gavage dose levels produced different responses. The evidence for bone marrow aberrations is considered very weak due to several negative studies including two micronucleus assays at equal doses.

By using a quantitative weight-of-evidence system developed by ICPEMC all test data including positive and negative studies could be evaluated for a consensus trend. The results of using this approach indicated that the consensus score for atrazine ranged from -16 to -20 suggesting that the positive responses were outliers and might be due to technical deficiencies or improper study designs. The weight-of-evidence analysis places atrazine in a non-mutagenic status relative to conventional health effects testing formats.

The issue of plant activation is not easily interpreted. However, a strong position can be taken that extrapolation of genotoxic activity from the plant activation studies to effects in mammals or mammalian cells has not been documented. Without such documentation one can only define a risk to plant somatic and germ cells from atrazine exposure. Studies in which plant activated intermediates or extracts from treated plants produced clear evidence of mutation in mammalian cells (in vitro or in vivo) will be required for

- 6 -

this extrapolation. The positive results for gene mutation and UDS in mammalian cells exposed to atrazine and potato microsomes are not sufficient without suitable documentation and confirmation.

Consequently, Dr. Brusick does not believe that an adequate case has been established supporting a presumption that atrazine is a genotoxic agent capable of initiating neoplasia or inducing transmissible mutation in mammalian germ cells.

Weight of Evidence Analysis

Decisions concerning whether a chemical may pose an oncogenic hazard to humans must be based on an evaluation of all relevant toxicological data.

The following items outline the broad range of information evaluated in a weight of evidence analysis.

- Epidemiology
- Long-Term Animal Studies
- Mutagenicity and Other Short-Term Tests
- Metabolic or Kinetic Studies
- Comparative Metabolism Studies
- Structure-Activity Relationships, and
- Mechanistic Investigations

In conducting the weight of evidence analysis for atrazine, the following toxicology observations were made.

Lifetime Rodent Studies

- A positive oncogenic effect was observed only in females of one strain of rat (mammary tumors in Sprague-Dawley strain).
- No oncogenic effects were observed in three strains of mice in three separate studies.

Short-Term Toxicity Tests

- No adverse results were noted in over 20-mutagenicity tests including tests for gene mutation, chromosomal aberration and primary DNA damage.

Human Experience

- There is no indication of a carcinogenic response in humans despite over 30 years of atrazine use.

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Mechanistic Considerations

- Experimental evidence indicates an alteration of prolactin and luteinizing hormone secretion in Sprague-Dawley female rats with mammary tumors. This is based on immunological staining of the pituitaries of the rat.
- The Sprague-Dawley rat has a spontaneous tumor incidence in the pituitary and mammary in excess of 50%. This supports the position that this rat is unusually sensitive to some endocrine effects.
- Epidemiological evidence does not support the supposition that chemicals which elicit mammary tumors in Sprague-Dawley rats have relevance to humans.
- Scientific experts and regulatory agencies have determined such hormonally-mediated responses in the rodent demonstrate a threshold.

CIBA-GEIGY believes EPA will classify atrazine as a Category C oncogen based on the results of the chronic rat study. Exposure to Category C oncogens may be regulated through either qualitative or quantitative (mathematical) risk assessment. CIBA-GEIGY contends that qualitative risk assessment and, therefore, a safety factor of 100-fold should be used to regulate exposure to atrazine. That belief is based on the results of the weight of evidence evaluation and the possibility that a threshold for oncogenicity will be demonstrated for atrazine.

Using a qualitative assessment allows calculation of margins of safety (MOS) for atrazine exposure for workers or the general population. CIBA-GEIGY calculations show that the worst case worker exposure would be approximately 900 times less than the NOEL of 0.7 mg/kg derived from the chronic rat study (the most sensitive species based on the results of the newly submitted long-term studies). The most likely case worker exposure would be approximately 27,000 times less than the NOEL. For the general population, worst case and most likely case exposure are approximately 20,000 to 50,000 times less than the NOEL derived from the 1986 chronic rat study. From these results and particularly if the most likely case MOS are used, it can be concluded that atrazine poses no threat to human safety.

R504MG0212LW

List of References for CIBA-GEIGY Submitted Data

1. Chronic Toxicity Study in Dogs (3 Vol.), EPA MRID No. 404313-01, Submitted November 17, 1987.
2. Oncogenicity Study in Mice (7 Vol.), EPA MRID No. 404313-02, Submitted November 17, 1987.
3. Two-Generation Rat Reproduction (6 Vol.), EPA MRID No. 404313-03, Submitted November 17, 1987.
4. Combined Chronic Toxicity/Oncogenicity Study in Rats (15 Vol.), EPA Acc. Nos. 262714-728. Submitted May 5, 1986.
5. Mouse Dominant Lethal Study (1 Vol.), EPA MRID No. 402466-03, Submitted June 19, 1987.
6. DNA Repair Test on Rat Hepatocytes (1 Vol.), EPA MRID No. 402466-02, Submitted June 19, 1987.
7. Dominant Lethal Assay in Mice, (1 Vol.), EPA MRID No. 402466-01, Submitted June 19, 1987.

R504MG0212LW

AN ASSESSMENT OF THE GENETIC TOXICITY
OF ATRAZINE: RELEVANCE TO HEALTH
AND ENVIRONMENTAL EFFECTS

PREPARED FOR
CIBA-GEIGY CORPORATION
AGRICULTURAL DIVISION

BY
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DECEMBER, 1987

3.1 (08#4)

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B.2

INTRODUCTION

Retrospective assessment of data from multitest mutagenicity testing has been performed using a variety of strategies. The simplest approach assumes a positive response in any of the composite assays is indicative of at least genotoxicity and possibly evidence for identification of the tested agent as mutagenic. Sufficient evidence for nonmutagenicity or nongenotoxicity would necessitate negative responses in all tests performed (1).

For most purposes, this approach to hazard assessment is too simple for the following reasons:

1. The mutagenic responses in specific assays with certain chemical classes are known to be erratic occasionally exhibiting positive effects for chemicals not considered to represent a genotoxic hazard (e.g., some nitroaromatic compounds in the Ames test [4]).
2. In vitro mammalian cell assays, measuring a variety of genotoxic endpoints, are known to produce "false positive" responses under severe treatment conditions. Significant reduction in the pH of the treatment medium or increases in osmolarity are both associated with these types of effects (49).
3. Several trials of the same test agent assay may yield divergent responses. Retesting the same material in a given assay may produce qualitatively different responses, especially if the magnitude of the effect in

the positive trials is marginal. Therefore, it may not be possible to resolve apparent conflicts in the biological activity by sequential repetition and data evaluation processes must be amenable with mixed test results.

4. Relevance of a test result is often determined by phylogenic complexity (i.e., in vivo mammalian effects are more convincing evidence of human hazard than positive results from submammalian in vitro assays). Positive in vitro results may not be significant in light of carefully conducted in vivo studies with negative results (2).
5. Likewise, some genetic endpoints carry greater relevance to genetic hazard than others. Sister chromatid exchange (SCE) is a good confirming test but does not carry much weight as a sole positive.

Therefore evaluation of multitest genetic toxicology analyses is more complex than counting positives and negatives. The process requires knowledge about a test's performance under a range of treatment conditions, general reproducibility of the assay, chemical class sensitivities of the test, metabolic relevance of the target organism for the test substance, and the confirmatory vs. complementary aspects of the test with other bioassays (7).

Atrazine in numerous forms and combinations has been tested for mutagenic activity since 1966 (8). The variety of techniques employed ranges from simple organisms such as bacteriophage to studies in mammals. Yet with this broad

range of experience over time and test systems, it has not been possible to reach a general consensus concerning the intrinsic genotoxicity of atrazine (5,6,33). Many factors have contributed to the complexity and include:

1. Tests conducted under what appear to be similar conditions with the same protocol yield inconsistent responses that cannot be explained from analysis of the reported data.
2. There appears to be differences in metabolism of atrazine between plants and animals; however, even within each of these two kingdoms differences may exist (seedlings vs. mature plants and insects vs. mammals).
3. Differences in the sample tested; and possibly the solvent or vehicle used may have significant influence on the results produced.

This evaluation is based on an interpretation of the composite data profile. Reviewing individual reports will not necessarily lead to a resolution of the issues. I have attempted to address each of the three problem areas identified above in order to determine whether a clear consensus or interpretation of the results can be derived. This process required the use of a weight-of-evidence data assessment technique. From such an analysis of all results it is possible to establish a general consensus as to whether the data trends toward positive results or not and, if so, to what extent.

Most weight-of-evidence approaches are qualitative but a quantitative method has been developed and studied over the past four years by the International Commission for Protection Against Environmental Mutagens and Carcinogens (ICPEMC). The ICPEMC scheme has been, in part, published (3,10) with additional manuscripts in preparation (47). This approach was designed to assess large data sets such as that available for atrazine. The method will be described in more detail later in this review.

STANDARD TESTING FORMAT (NONPLANT)

The issues involved in this assessment concern the intrinsic genotoxicity of atrazine in vivo or in vitro. The target cells and metabolism are used with the intent of establishing hazard potential for humans via direct exposure to atrazine. Somatic and germ cells if altered by genotoxic agents may result in nonreversible toxicity (e.g. cancer, mutation, terata). Screening and hazard assessment tests are applied in batteries to determine mutagenic potential.

1. Selection of Studies Available for Evaluation

The tests selected from this testing format included those conducted in vitro using atrazine directly or with an exogenous activation system such as S9 mix or in vivo. The range of assays was comprehensive and included tests that would permit a reasonably good hazard assessment to be made under most circumstances. Several of the tests were conducted multiple times adding potentially valuable information regarding reproducibility and the possible influence of different routes of exposure and different laboratory environments. A listing of studies included is given in Table 1.

TABLE 1
Studies Evaluated Using Either a Weight-of-Evidence
Approach or a Test Resolution Approach

Test	References
Rec Assay	16
Ames	9,11,16,44
Host-mediated Assay (Salmonella)	11,33
Host-mediated Assay (Yeast)	33
Host-mediated Assay (E. coli)	33
SCE in Cultured Human Lymphocytes	42
SCE in CHO Cells	33
Chromosome Aberrations in CHO Cells	33
HGPRT Mutation Assay in V79 Cells	33
Drosophila SLRL	31,32,33
UDS in Rat Hepatocyte Cultures	40
UDS in Cultured Human Fibroblasts	41
Mouse Bone Marrow Metaphase Analysis	33,45
Mouse Dominant Lethal Assay	33,34,45
Aberrations in Mouse Germ Cells	38
Chinese Hamster Nucleus Anomaly Test*	35
Mouse Somatic Mutation Assay (Spot Test)	52,19
Mouse Spermhead Abnormality Assay	46
SCE in Normal Chinese Hamster Kidney Cells	50
SCE <u>In Vivo</u> in Chinese Hamster	53
Chinese Hamster Bone Marrow Chromosome Aberrations	53
Mouse Bone Marrow Micronucleus Test	54

*Essentially equivalent to a micronucleus assay

This list contains those studies believed to be of some value in assessing the genetic toxicity of atrazine. Eliminated from the list are studies in bacteriophage T4, Ames tests conducted using spot test methods, and some studies described only in qualitative terms (no data). The reasons for selectively excluding these methods is that they are not common techniques with adequate historical data bases or sufficient multilaboratory experience.

2. Methods of Evaluation

Two evaluation methods were used to evaluate the assays considered in the standard testing format.

a. Weight-of-Evidence Approach

The first method is the quantitative weight-of-evidence scoring system developed by ICPEMC designed specifically to handle large, complex data sets with inconsistent responses (10).

A weight-of-evidence approach integrates the responses, both positive and negative, from all tests conducted, considering the multiple variables cited above and determines the net direction (positive or negative) of the evidence. It is amenable to mixed results as long as a trend of responses can be established. The following are characteristics of this approach:

- (1) In vivo results tend to dominate in vitro responses especially positive in vivo effects.

- (2) Replicated tests assume greater weight than those performed only once.
- (3) Because gene mutation and chromosome aberration effects represent known etiology for human genetic toxicity, they tend to predominate over SCE, gene conversion or DNA repair data which cannot be attributed directly to human disease.
- (4) Because it is impossible to prove a negative, a small number of positives will generally provide a greater weight-of-evidence than an equivalent or possibly larger number of negative responses.
- (5) Positive results generated at low exposure levels have greater weight than positive results generated at high concentrations or at the maximum tolerated dose. Increased weight for negative results requires a relationship to doses applied that is the inverse of that for positives.

A weight-of-evidence analysis gains power as the number of tests increase because a clear, stable trend should begin to emerge.

The initial version of the ICPEMC method was published in 1986 (10). During the past two years the system has been refined (3) and tested against sets of data from more than 100 chemicals. In addition, a computer program to process the data and generate graphic displays of the results has been produced. This more extensive validation data using the method will be published in 1988 (47).

The weight-of-evidence technique developed by ICPEMC has the following characteristics:

1. It is quantitative, reducing complex multitest data to a single consensus score. Similar data sets analyzed at different times will be evaluated in a consistent fashion.
2. It is open-ended with respect to the number and type of tests analyzed. The minimum required is five, with at least three in vitro and two in vivo. However, two chemicals subjected to different short-term assays can be directly compared using this pattern.
3. It evaluates both positive and negative responses, includes and estimation of potency, gives weight to replication of tests and gives greatest value to in vivo positive responses.
4. Each level of data entry and data reduction are graphically expressed in order to demonstrate the contribution of each test and group of tests to the final integrated consensus score. Estimates of data variance are provided to add perspective to the quantitative estimates.

The method also provides that independent sets of data are subjected to the same analysis criteria. Consequently, one can generate objective evaluations of data from compounds that have been screened with similar or different test batteries. It must be noted, however, that the computer program does not critique or censor the input data and cannot compensate for poor technical quality.

b. Response Resolution Technique

The second approach was done to complement the weight-of-evidence approach. It reviews each study and where conflicting data exist attempts to resolve the conflicts via protocol comparisons or other technical aspects of the studies. Once the conflicts are resolved, a form of weight-of-evidence was used on the reviewed data set excluding the results considered spurious or otherwise not relevant to a hazard assessment. This approach proved to be difficult when conflicting responses could not be resolved due to insufficient information in the report or protocol designs were unclear. Thus, the primary differences between the two approaches is a proactive attempt to resolve some data sets prior computer weighting.

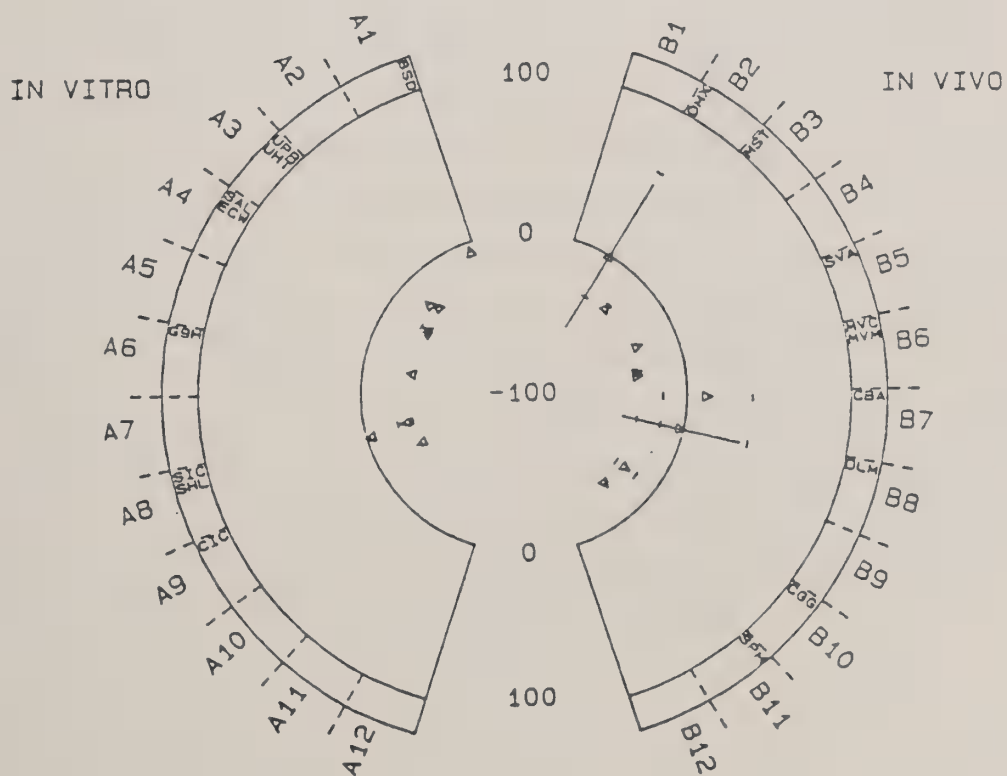
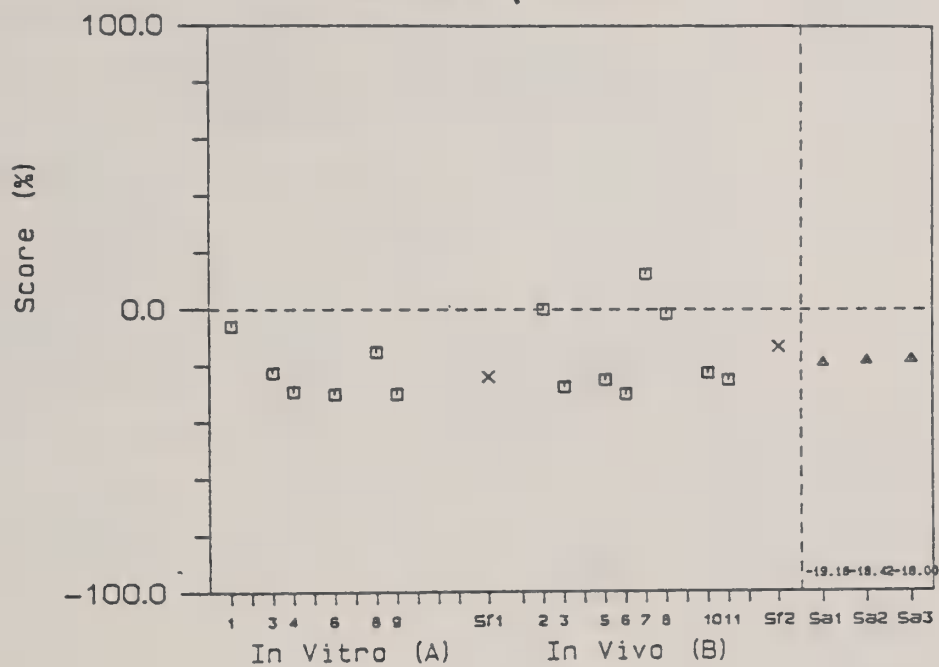
3. Results

a. Quantitative Weight-of-Evidence Profile

Figure 1 is an ICPEMC plot of in vitro and in vivo data reported for atrazine in the standard testing format. The data have been merged to class, family and agent scores by the ICPEMC approach. The figure shows that atrazine has a clearly negative agent score (S_a) regardless of how the S_a score is calculated (i.e., S_{a1} , S_{a2} or S_{a3}). The S_a score represents the consensus of all data entries. Once the data has been merged into class scores (upper portion of Figure 1) there is only a single test class which registers positive and that is the bone marrow metaphase analysis. However, the trend bulk of

FIGURE 1

Date : 12/13/1987 Chemical = ATRAZINE
Cas number = 1912249
Matrix = 1



the test data indicates that atrazine, is not a mutagen. The weight of the positive bone marrow has been reduced by negative studies of the same type as well as other cytogenetic studies for SCE and micronucleus that were also negative. This process of data evaluation, therefore, accommodates the occasional positive response but the consensus is based on a composite of all study responses adjusted for test replication and potency. The consensus score for atrazine is strongly negative and because of the large number of tests is most likely an accurate reflection of the intrinsic biological properties of this chemical. Atrazine positive studies probably represent spurious effects or technical problems encountered in resolving data.

b. Response Resolution Approach

Tables 2-5 provide a resolution of the conflicting data sets (in this format) reported for atrazine. The results of this approach are similar but not identical to the quantitative weight-of-evidence method. The first difference is that host-mediated assays were not evaluated in the ICPEMC scheme primarily because of their known unreliability. The second difference is that in data conflict situations the ICPEMC approach merges all entries where as the conflict resolution approach seeks to establish a "right" response and eliminate other responses from consideration. Consequently, in addition to the mouse bone marrow metaphase assay appearing positive (as shown in Figure 1), the mouse dominant lethal assay was resolved in favor of the positive study (Table 5). Thus, atrazine appeared

TABLE 2
Evaluation of Conflicting Responses in
Mouse Bone Marrow Metaphase Analysis

Positive Study(s)	Negative Study(s)	Resolution
An oral gavage exposure to 2000 mg/kg of atrazine prepared in olive oil (ref. 33).	<p>An acute i.p. injection of 6 mg/kg atrazine. Details of the study were lacking since the citation is an abstract (Mutation Res. 97:237, 1982). Drinking water exposure of 1 ppm of atrazine for 7 weeks was also negative.</p> <p>Acute oral gavage administered in corn oil at 2000 mg/kg. Bone marrow cells evaluated for micronuclei (54). A second micronucleus analysis in Chinese hamsters was performed by Ciba-Geigy (35). A small study conducted in Chinese hamster bone marrow (500 mg/kg) was negative (53).</p>	<p>The studies appear different in both route of exposure and dose level applied.</p> <p>Insufficient information was available to exclude any set of results. Consequently, the positive response at 2000 mg/kg appears acceptable. It should be noted, however, that the predominant findings were simple (chromatid) breaks and not complex aberrations. A Chinese hamster bone marrow micronucleus assay was negative at 1128 mg/kg and a mouse bone marrow micronucleus assay at 2000 mg/kg was negative.</p> <p>The two micronucleus and the hamster bone marrow assays at roughly equivalent dose levels do not totally offset the positive bone marrow metaphase assay because (a) the metaphase assay is generally considered to be more sensitive, and (b) the negative metaphase analyses were conducted at equivalent or lower doses.</p>

TABLE 3

Evaluation of Conflicting Responses in
Drosophila Sex-Linked Recessive Lethal Assays

Positive Study(s)	Negative Study(s)	Resolution
<p>Atrazine was placed in the larvae food supply at approximately 100 μg per ml and produced increased lethals (ref. 31).</p> <p>Atrazine was injected into male flies using aliquots of the same stock (100 $\mu\text{g}/\text{ml}$) and produced increased lethals (ref. 31).</p>	<p>An adult feeding study at 10,000 $\mu\text{g}/\text{ml}$ was negative (ref. 32).</p> <p>An adult feeding study at 2,160 $\mu\text{g}/\text{ml}$ was negative (ref. 33).</p>	<p>The positive studies employed small sample sizes (~1000 flies per dose) which would increase the variability of the spontaneous background frequencies. The results were then compared to a much larger non-concurrent historical control sample with less variance around the background frequency. In my opinion the positive studies are not valid. It may be larvae feeding or ingestion is needed to produce an effect but neither of these positive studies will withstand a vigorous critique. The results from the two adult feeding studies, especially the one in ref. 33, are more reliable measures.</p>

TABLE 4
Evaluation of Conflicting Responses in
Microbial Host-Mediated Assays

Positive Study(s)	Negative Study(s)	Resolution
<p><u>S. cerevisiae</u> strain D4 (Loprieno <i>et al.</i>) was positive. The test organisms were injected intratesticular into male rats exposed to 1 g/kg of atrazine.</p> <p><u>E. coli</u>; an ampicillin sensitive strain was used in a conventional host-mediated assay. The results at 100 µg/kg were reported positive (33).</p>	<p><u>S. typhimurium</u> strains TA-1535 and TA-1538 were tested up to 2,200 mg/kg in a conventional host-mediated assay. The results were negative (11). A second study using <u>S. typhimurium</u> was reported qualitatively to be negative (33).</p>	<p>In general host-mediated studies are not reliable indicators of genotoxicity because of extreme variability in cell recovery from the host and because of possible interactions between the host and the injected target cells. The studies with yeast D4 must be viewed with caution since the cells were injected into the testes. Very little is known about possible artifact formation or selective cell recovery. This protocol is also specific to a single laboratory. The second positive study uses an antibiotic resistance marker. Antibiotic resistance is not a good marker for mutation because it is leaky and phenocopies may develop. The <u>Salmonella</u> studies are based on protocols proven to be reasonably reliable in multiple laboratories and I would consider the negative results to be more accurate in this case.</p>

TABLE 5

Evaluation of Conflicting Responses in
the Mouse Dominant Lethal Assay

Positive Study(s)	Negative Study(s)	Resolution
Dominant lethal effects were observed in mice exposed to 1,500 and 2,000 mg/kg of atrazine by oral gavage (33). Olive oil was used as the vehicle.	No effects were observed in mice exposed acutely to 6 mg/kg atrazine by i.p. injection or to 1 ppm administered in drinking water for seven weeks (Mutation Res. 99:237, 1982). A second negative study was reported for atrazine at 1332 mg/kg administered by oral gavage. CMC was used as the vehicle (34).	The positive effects were reported at two dose levels higher than the applied doses of either of the two negative studies. Since there were no other confounding factors, the positive results are accepted.

capable of producing chromosome aberrations in vivo including male germ cells. Reliability of the chromosome aberration effects are significantly diluted by the bulk of other data. The remainder of the data base is negative or resolved in favor of negative studies.

Some data were not used in either evaluation. Bacteriophage test results were negative but one cannot easily place a relevance value on this study type. Other negative responses from yeast and fungal studies (*Aspergillus*) were provided qualitatively in abstracts. Without some sort of dose quantitation, these results are of little value as negative entries.

TESTING IN PLANTS OR TESTS USING PLANT CELL ACTIVATION

The issues raised by this aspect of the evaluation are that atrazine is biologically active in plants and these organisms may well metabolize the herbicide to products which are not only different than those produced in animal cells but which may be potentially hazardous to the environment or to individuals consuming treated crops. Positive studies in plant systems were reported as early as 1966 (8). These studies were limited to chromosome damage in sorghum and barley root tips (21,36). Root tip studies are notoriously unreliable because secondary effects such as changes in pH or osmotic condition and not the test agent, may well be responsible for chromosome breakage. Other studies with atrazine fail to confirm the induction of chromosome abnormalities in plants (20).

1. Plant Cell Activation of Atrazine

Studies first reported by Gentile and Plewa in 1975 (22,23,26) and extended through 1984 demonstrated that atrazine induces genotoxicity via assays designated in vitro, in vivo and in situ (25). In vitro assays involve the preparation water soluble plant extracts which activate atrazine to cell culture and microbial genotoxins (5,33). In vivo assays involve growing plants in an environment exposed to atrazine followed by a demonstration that homogenates the plants grown in the presence of atrazine are mutagenic to microorganisms. The in situ assays demonstrate genetic damage directly to the germ cell line of the exposed plants.

2. Implication of Plant Cell Activation and Target Cell Responses

Several points related to these studies are important in establishing the relevance of these studies to human health or environmental toxicity:

1. The mutagenic species are not produced by mammalian microsomes used in vitro but are produced by plant microsomes (5,33). This should reduce concern for atrazine produced genotoxicity in mammals.
2. Microsomes from plant cells of different maturity appear to have different effects on atrazine bioactivation. The studies of Plewa and Gentile and coworkers which showed positive effects in the Ames assay or yeast mitotic gene conversion (strain D₄) were conducted with exposed young corn seedling tissue. Mutagenicity test results with plant cell extracts from mature leaves of treated and untreated

corn plants were found to be independent of atrazine exposure (27,28). Factors other than atrazine were suggested as factors influencing plant extract genotoxicity.

3. The ability of the plant cell derived mutagens to produce clear effects in mammalian cells is limited, at best (33). The in vitro studies cited by Adler (33) that were conducted in Dr. Loprieno's laboratory with potato microsomes provide insufficient evidence for activity. At the present time, none of the actual data from these studies has been available for peer review. They must be peer reviewed and confirmed by independent trials before the qualitative assessments are factored into hazard assessments. Consequently, there is no substantiated evidence suggesting that consumption of atrazine-treated plant tissue has any genotoxic activity that represents hazard potential for humans.

While it may be true that atrazine has the ability to induce germ cell damage in plants (5,36,37) exposed in situ, this cannot be equated with evidence for human health hazards. The evidence for plant activation is variable and may be limited to seedling tissue or possible cultured plant cell extracts (3,29). The use of potato microsome preparations (33) clearly needs further assessment. Their use has not been adequately validated, and considering the problems associated with auto oxidation and generation of peroxidation products by rat liver microsomes one should not be too quick to attribute in vitro positive responses to the test chemicals. Work with

plant cell activation is continuing (30,48) and may eventually yield some answers concerning environmental toxicity and possibly health effects but at the toxicity and possibly health effects but at the present time this information is too limited and variable to reach any definitive conclusions.

POSSIBLE ROLES OF ATRAZINE SOURCE, VEHICLE, OR ROUTE OF ADMINISTRATION

1. Source of Atrazine

Although the information regarding source and purity of atrazine across the studies examined was limited, there did not seem to be an association of genotoxicity with source or level of purity. Several studies in which effects were observed with combined exposures (i.e. atrazine plus 2,4-D, alachlor or other triazines) were not particularly informative because of the alleged genotoxicity of the companion products.

2. Vehicle Effects

There were differences in effects that might be attributed to vehicle. For example, in the positive mouse dominant lethal study, atrazine was administered in olive oil (33). Another study in mice using an almost equivalent dose level of atrazine administered in CMC was completely without evidence of dominant lethal effects (34). Olive oil was also used as vehicle in the positive bone marrow metaphase analysis. Atrazine administered to Chinese hamsters in CMC at almost an equivalent dose failed to induce chromosome damage in bone marrow. A closer inspection of olive studies might be valuable. Also the bioavailability of atrazine from CMC might be investigated.

3. Route of Administration and Test Response

The only indications that route of exposure may be important in a test response come from *Drosophila* sex-linked recessive lethal (SLRL) results alleging that atrazine fed to larvae or injected into larvae was genetically active whereas atrazine fed to adult flies was inactive (31,32,33).

The possibility exists that any or all of these factors may play a determining role in a test response; however, that data are insufficient to draw a defensible conclusion. Thus, without further studies one cannot exclude specific studies from consideration based on sample source or purity, vehicle or route of administration.

CONCLUSIONS

The objectives of this evaluation were to (1) reconcile the inconsistencies of test results within the data base, (2) interpret the significance of the possible different bioactivation of atrazine in plants and animals, and (3) draw a conclusion regarding the possible genetic toxicity of atrazine to humans.

It is not scientifically possible, nor totally necessary to reconcile all the test-response conflicts for atrazine. In some cases such as the *Drosophila* SLRL assays it is relatively easy to dismiss the studies in which sample sizes varied and nonconcurrent negative controls were used. In other cases, such as the mouse dominant lethal studies, it appears that two studies using approximately equivalent gavage dose levels produced different responses. The evidence for bone marrow aberrations is considered very weak due to several negative studies including two micronucleus assays at equal doses.

By using a quantitative weight-of-evidence system developed by ICPEMC all test data including positive and negative studies could be evaluated for a consensus trend. The results of using this approach indicated that the consensus score for atrazine ranged from -16 to -20 suggesting that the positive responses were outliers and might be due to technical deficiencies or improper study designs. The weight-of-evidence analysis places atrazine in a non-mutagenic status, in my opinion, relative to conventional health effects testing formats.

The issue of plant activation is not easily interpreted. However, a strong position can be taken that extrapolation of genotoxic activity from the plant activation studies to effects in mammals or mammalian cells has not been documented. Without such documentation one can only define a risk to plant somatic and germ cells from atrazine exposure. Studies in which plant activated intermediates or extracts from treated plants produced clear evidence of mutation in mammalian cells (in vitro or in vivo) will be required for this extrapolation. The positive results alleged by Loprieno (33) for gene mutation and UDS in mammalian cells exposed to atrazine and potato microsomes are not sufficient without suitable documentation and confirmation.

Consequently, I do not believe that an adequate case has been established supporting a presumption that atrazine is a genotoxic agent capable of initiating neoplasia or inducing transmissible mutation in mammalian germ cells.

APPENDIX

QUANTITATIVE WEIGHT-OF-EVIDENCE
CALCULATIONS AND METHODS FOR INTERPRETATION

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The following information is attached for interpretation of Figure 1 found on page 10 of the text.

1. Listing of tests by code name and test class designation. This will permit one to identify tests located in the lower portion by three letter codes and classes listed in the upper portion of the figure by numbers.
2. A series of calculations showing the transformation of data assessed in this evaluation. The actual calculation methods will be provided in detail in Brusick et al., (47).
3. A version of Figure 1 with additional information needed to read the profile.

THE CURRENT FAMILY, CLASS AND TEST STRUCTURE**

A: IN VITRO FAMILY

Class 1: Primary DNA damage - prokaryotes.

- BSD *Bacillus subtilis* rec strains, differential toxicity.
 ECD *Escherichia Coli* pol A/W3110-P3478 spot test, differential toxicity.
 ECL *Escherichia Coli* pol A/W3110-P3478 liquid suspension test, differential toxicity.
 ERD *Escherichia Coli* rec strains, differential toxicity.

Class 2: Primary DNA damage - lower eukaryotes.

- SCG *Saccharomyces cerevisiae*, gene conversion.
 SCH *Saccharomyces cerevisiae*, homozygosis by recombination or gene conversion.

Class 3: Primary DNA damage - mammalian cells.

- UHF Unscheduled DNA synthesis in human fibroblasts *in vitro*.
 UHT Unscheduled DNA synthesis in transformed human cells *in vitro*.
 UIH Unscheduled DNA synthesis in other human cells *in vitro*.
 UPR Unscheduled DNA synthesis in rat primary hepatocytes *in vitro*.
 ULA Unscheduled DNA synthesis in other animal cells *in vitro*.

Class 4: Gene mutation - prokaryotes.

- ECZ *Escherichia Coli* WP2, reverse mutation.
 ECW *Escherichia Coli* WP2 *uvrA*, reverse mutation.
 ECR *Escherichia Coli*, miscellaneous strains, reverse mutation.
 SAL *Salmonella typhimurium*, all strains.

Class 5: Gene mutation - lower eukaryotes.

- NCF *Neurospora crassa*, forward mutation.
 NCR *Neurospora crassa*, reverse mutation.
 SCF *Saccharomyces cerevisiae*, forward mutation.
 SCR *Saccharomyces cerevisiae*, reverse mutation.
 SZF *Schizosaccharomyces pombe*, forward mutation.
 SZR *Schizosaccharomyces pombe*, reverse mutation.

Class 6: Gene mutation - mammalian cells.

- GST Gene mutation in mouse lymphoma cells LS178Y, TK locus.
 GSI Gene mutation in mouse lymphoma cells LS178Y, other loci.
 GML Gene mutation in mouse lymphoma cells other than LS178Y.
 G9H Gene mutation in Chinese hamster lung cells V-79, HPRT.
 G9O Gene mutation in Chinese hamster lung cells V-79, ouabain.
 GCL Gene mutation in Chinese hamster lung cells other than V-79.
 GCO Gene mutation in Chinese hamster ovary cells.
 GLA Gene mutation in other animal cells.

Class 7: Aneuploidy - lower eukaryotes.

- NCN *Neurospora crassa*, aneuploidy.
 SCN *Saccharomyces cerevisiae*, aneuploidy.

Class 8: Sister chromatid exchange - mammalian cells.

- SHF Sister chromatid exchange in human fibroblasts.
 SHL Sister chromatid exchange in human lymphocytes.
 SIH Sister chromatid exchange in other human cells.
 SIC Sister chromatid exchange in Chinese hamster cells.
 SIA Sister chromatid exchange in other animal cells.
 SIT Sister chromatid exchange in transformed cells.

Class 9: Chromosome aberration - mammalian cells.

- CHL Chromosome aberration in human lymphocytes.
 CIC Chromosome aberration in Chinese hamster cells.
 CIS Chromosome aberration in Syrian hamster embryo cells.
 CAL Chromosome aberration in animal leukocytes.
 CIA Chromosome aberration in other animal cells.
 CIT Chromosome aberration in tumor cells.

Class 10: Transformation - mammalian cells.

- TBM Transformation in BALB/C3T3 mouse cells.
 T7R Transformation in SA7/rat cells.
 TRR Transformation in RL/V/FISCHER rat embryo cells.
 T7S Transformation in SA7/SHE cells.
 TCM Transformation in Syrian hamster embryo cells, clonal assay.
 T7S Transformation in Syrian hamster embryo cells, focus assay.
 TCL Transformation in other established cell lines.

B: *IN VIVO* FAMILY

Class 1: Somatic DNA repair - mammal.

UVH Unscheduled DNA synthesis in humans.
UVC Unscheduled DNA synthesis in hamsters.
UVM Unscheduled DNA synthesis in mice.
UVR Unscheduled DNA synthesis in rats.
UVA Unscheduled DNA synthesis in other animals.

Class 2: Gene mutation - insect, *Drosophila*.

DMH *Drosophila melanogaster*, heritable translocation test.
DML *Drosophila melanogaster*, dominant lethal test.
DMM *Drosophila melanogaster*, somatic mutation.
DMX *Drosophila melanogaster*, sex-linked recessive lethal test.

Class 3: Somatic spot test - mammal.

MST Spot test, mouse.

Class 4: Heritable specific locus test - mammal.

SLO Specific locus test, mouse, other stages.
SLP Specific locus test, mouse, postspematogonia.

Class 5: Sister chromatid exchange, somatic - mammal.

SVH Sister chromatid exchange in humans.
SVA Sister chromatid exchange in animals.

Class 6: Micronuclei, somatic - mammal.

MVC Micronucleus test in hamsters.
MVM Micronucleus test in mice.
MVR Micronucleus test in rats.
MVA Micronucleus test in other animals.

Class 7: Chromosome aberration, somatic - mammal.

CBH Chromosome aberration in humans, bone marrow.
CLH Chromosome aberration in humans, lymphocytes.
CVH Chromosome aberration in humans, other cells.
CBA Chromosome aberration in other animals, bone marrow.
CLA Chromosome aberration in other animals, leukocytes.
CVA Chromosome aberration in other animals, other cells.

Class 8: Dominant lethal - mammal.

DLM Dominant lethal test in mice.
DLR Dominant lethal test in rats.

Class 9: Heritable translocation - mammal.

MHT Heritable translocation test in mice.

Class 10: Chromosome aberration, germinal - mammal.

CCC Chromosome aberration in spermatocytes, treated and observed.
CGC Chromosome aberration in spermatogonia treated, spermatocytes observed.
CGG Chromosome aberration in spermatogonia, treated and observed.
COE Chromosome aberration in oocytes or embryos.

Class 11: Sperm morphology - mammal.

SPH Sperm morphology in humans.
SPF Sperm morphology in F1 mice.
SPM Sperm morphology in other mice.
SPR Sperm morphology in rats.
SPS Sperm morphology in sheep.

** Code names according to Waters et. al., 1987.

ATTACHMENT		1912249		Date : 12/13/1987		Page : 7				

				* I C P E M C *						

TEST USE WA	DOSE	Matrix = 1			### IN VITRO ###					
		Hp	Ia	Ib	Icd	Re	Rs			
BSD - 1	10000.0000 :	3.0	0.5	0.2	0.6 :	-0.13	-6.00	1 0.6	1 0.4	6016 :
UPR - 0	150.0000 :	4.0	0.5	1.0	0.5 :	-1.00	-25.00	1 0.6		6040 :
UIT - 1	150.0000 :	4.0	0.5	0.3	0.5 :	-0.13	-20.00	1 0.6	2 0.7	6041 :
SAL - 0	10000.0000 :	7.0	0.5	1.0	0.6 :	-2.10	-30.00			6016 :
SAL - 0	5000.0000 :	7.0	0.5	1.0	0.6 :	-2.10	-30.00			6011 :
SAL - 0	310.0000 :	7.0	0.5	1.0	0.5 :	-1.75	-25.00			6009 :
SAL - 0	5000.0000 :	7.0	0.5	1.0	0.6 :	-2.10	-30.00	2.50	4 1.0	6044 :
ECM - 0	5000.0000 :	7.0	0.5	1.0	0.6 :	-2.10	-30.00	1 0.6		6016 :
GPH - 0	2160.0000 :	7.0	0.5	1.0	0.6 :	-2.10	-30.00	1 0.6	2 0.7	6033 :
STC - 0	2160.0000 :	6.0	0.5	1.0	0.6 :	-1.80	-30.00		1 0.4	6033 :
STC - 0	70.0000 :	6.0	0.5	1.0	0.4 :	-1.20	-20.00	2 0.8		6050 :
SRL - 1	0.1000 :	6.0	0.5	0.4	0.1 :	-0.12	-2.00	1 0.6		6042 :
CIC - 0	2160.0000 :	7.0	0.5	1.0	0.6 :	-2.10	-30.00	1 0.6	2 0.7	6033 :
									1 0.4	

ATRAZINE

1212249

Date : 12/13/1987

Page 8

* I C P E M C *

TEST WS	DOSE	Matrix = Hp Fg	Fd	Re	Rs	Te	Ts	Tv	Tn	Rp	Cl	Ce	## IN VIVO ##	Cv	Cn	Im	REFERENCE
DMX +	100.0000 :	6.0 1.0	0.6 :	3.60 60.00													6031 :
DMX 0	10000.0000 :	6.0-0.5	0.6 :	-1.80 -30.00													6032 :
DMX 0	2160.0000 :	6.0-0.5	0.6 :	-1.80 -30.00		0.00	0.00	51.96	3 0.9		2	0.00	0.00		1 0.4		6033 :
HST 0	1250.0000 :	3.0-0.5	0.6 :	-2.40 -30.00		-1.76	-27.50		2 0.8		3	-0.70	-27.50		1 0.4		6019 :
HST 0	500.0000 :	6.0-0.5	0.5 :	-2.00 -25.00		-0.90	-25.00		1 0.6		5	-0.36	-25.00		1 0.4		6052 :
SVA 0	500.0000 :	6.0-0.5	0.5 :	-1.50 -25.00		-1.08	-30.00		1 0.6								6053 :
HVC 0	1128.0000 :	6.0-0.5	0.6 :	-1.80 -30.00		-1.03	-30.00		1 0.6		6	-0.76	-30.00				6035 :
MVH 0	2000.0000 :	6.0 0.5	0.6 :	-1.80 -30.00											2 0.7		6054 :
CBA 0	6.0000 :	3.0-0.5	0.3 :	-1.20 -15.00		0.80	12.50		2 0.8		7	0.32	12.50				6045 :
CBA +	2000.0000 :	6.0 1.0	0.4 :	3.20 40.00											1 0.4		6033 :
DLM 0	6.0000 :	3.0-0.5	0.3 :	-0.45 -15.00		-0.04	-1.67	36.86	3 0.9		8	-0.02	-1.67		1 0.4		6045 :
DLM +	1500.0000 :	3.0 1.0	0.4 :	1.20 40.00													6033 :
DLM 0	1332.0000 :	3.0-0.5	0.6 :	-0.90 -30.00		-0.54	-22.50		2 0.8		10	-0.22	-22.50		1 0.4		6034 :
CGG 0	6.0000 :	3.0-0.5	0.3 :	-0.45 -15.00		-0.15	-25.00		1 0.6		11	-0.06	-25.00				6045 :
CGG 0	1332.0000 :	3.0-0.5	0.6 :	-0.90 -30.00											1 0.4		6038 :
SPH 0	600.0000 :	1.0-0.5	0.5 :	-0.25 -25.00													6047 :

IN VITRO	Fe	FS	IN VIVO	Fe	FS	AGENT	Ca	Sal			Sd2			Sd3			
	-0.50	-23.93		-0.22	-13.27	-0.36	-19.18				-18.42			-18.00			

Date : 10/19/1987 Chemical = ATRAZINE
 Cas number = 001912249
 Matrix = 1

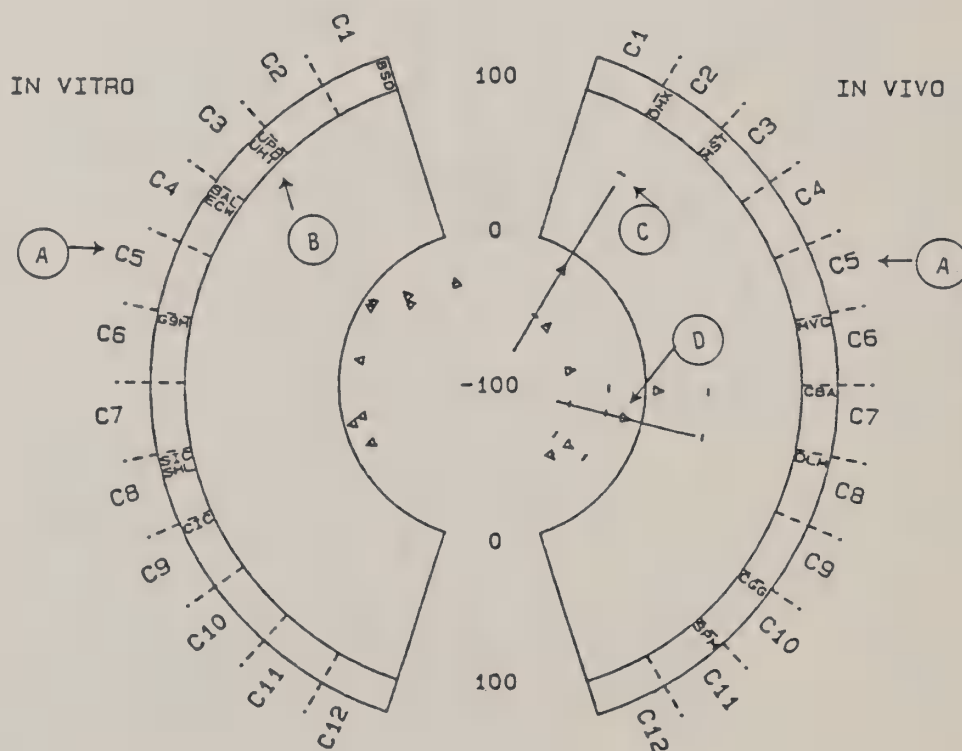
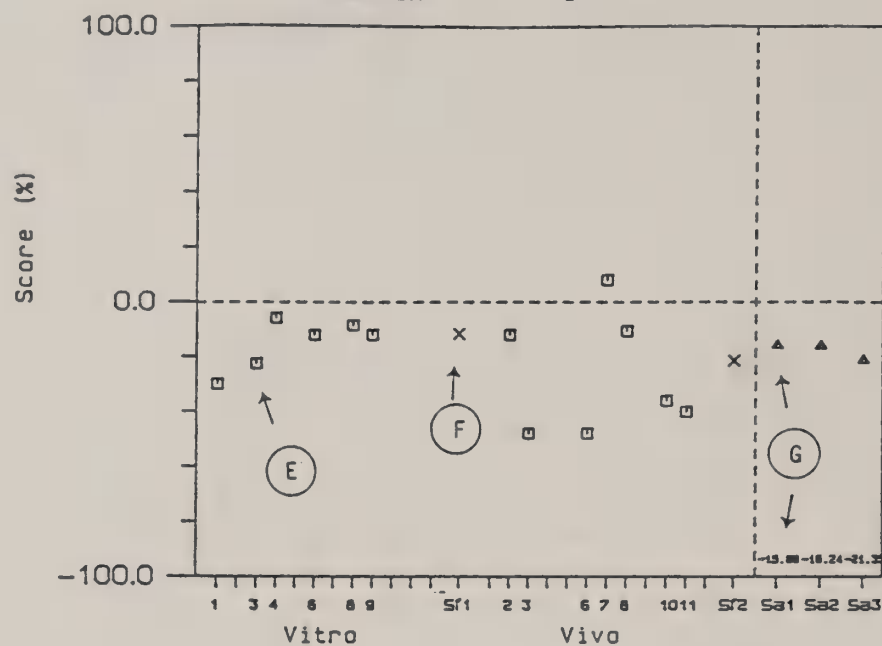


FIGURE EXPLANATION

Example Symbol	Label	Description
C5	A	Identification of <u>in vitro</u> or <u>in vivo</u> test classes. A test class contains individual tests related by genetic endpoint (see Appendix Table 1).
UPR	B	Three letter identifying specific tests (see Appendix Table 1).
-	C	Designation for a test entry score (-100 to +100 range).
A	D	Designation for the mean test score (equal to the test score if only a single entry). Error bars are given for three or more entries.
□□	E	Designation for a class score (i.e. mean of all test scores in a class).
X	F	Family score (mean of all class scores for <u>in vitro</u> or <u>in vivo</u> family).
Δ	G	Agent score designation. The actual calculated score Sa_1 , Sa_2 or Sa_3 is listed.

001004

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December 24, 1987

Certified Mail
Return Receipt Requested

Gary Larsen
Vegetation Management Interdisciplinary Team Leader
USDA, Forest Service
Pacific Northwest Region 6
P.O. Box 3623
Portland, Oregon 97208

RE: Draft EIS on managing competing and unwanted vegetation.

Dear Mr. Larsen:

These comments on the above-mentioned draft EIS are submitted on behalf of Citizens for Environmental Quality, the Idaho Natural Resources Legal Foundation, and myself. We appreciate the opportunity to participate in developing your vegetation management EIS.

We respectfully request that the comments contained herein be treated as substantive within the meaning of the Council for Environmental Quality regulations implementing the National Environmental Policy Act [40 CFR 1503.41]. Citizens for Environmental Quality and the Legal Foundation are public interest organizations dedicated to the preservation and protection of Idaho's natural resources. We have historically been and continue to be active in vegetation management issues on federal lands. We are members of the Idaho Noxious Weed Workgroup which has been responsible for major policy changes at the regional level regarding how weeds are controlled on Forest Service and BLM lands.

Although the draft EIS has developed a range of alternatives that acknowledge the need for timber production and protection of human health, only a cursory examination has been given to noxious weed control. Therefore, the document is seriously flawed by its failure to: (1). devote "substantial treatment" to noxious weed control [40 CFR 1502.14] and (2). provide the basic background information necessary for future "tiering" of site-specific EA's [40 CFR 1502.20].

These flaws can be corrected by adopting and incorporating the Intermountain Region noxious weed policy and prioritization plan into the final EIS. (Exhibit 1: J. S. Tixier, Regional Forester's Record of Decision, December 15, 1987).

Rational for Incorporating the Intermountain Region Noxious Weed Policy and Prioritization Plan: It has only been in the last few years that any attempts have been made to develop integrated pest management(IPM) principles which are practical for application in noxious weed control programs on federal lands.

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In fact, the Intermountain Region is the first and only Forest Service region in the nation which has developed and plans to maintain an aggressive IPM program specific to controlling noxious weeds on lands within their jurisdiction. Based on recent policy changes, it is the intent of the Regional Forester to implement noxious weed IPM on approximately 31 million acres of National Forest System lands in southern Idaho, Utah, Nevada, western Wyoming, and portions of California and Colorado.

The Regional plan, dynamic in scope, provides a basic IPM framework specific to noxious weeds. It includes education and awareness, planning for noxious weeds, monitoring (mapping & surveying), identifying and preventing conditions which cause noxious weeds.

Contrary to the draft EIS's claim that "general priorities for control should be established at the forest level"(DEIS,p.G-11), the Regional plan sets treatment priorities for noxious weed infestations on Forest Service lands. The different priorities reflect the Regional Forester's position that action should be directed first at education or making land managers aware of noxious weed species that do not yet occur in their specific management jurisdiction (potential new invaders), second at halting the spread of specific noxious weeds by eradicating or controlling small infestations of new invaders, and third at managing and reducing larger and well-established infestations. These different priorities do not list specific weed species but simply assign goals and components of control. The Regional Forester's approach recognizes that a Priority II species in one area may not be a Priority II species in another area. Therefore, specific species assignments are left up to each Forest in cooperation with county weed control authorities. A brief summary of the priorities are as follows: (EXHIBIT 1)

Priority I: Potential New Invaders: These are noxious weeds which are not on a Forest(s) yet, but the potential for infestation is imminent. Emphasis is placed on awareness and education so that when a new infestation occurs, it is identified and immediate action taken to eradicate or control the infestation while the noxious weed population is low.

Priority II: New Invaders: The highest priority in treatment and funding is being directed at small or isolated infestations which are not currently established on a Forest. A key factor in treating Priority II weeds is to prevent conditions which allow them to become established. Eradication is the goal for weeds of this priority, if feasible.

Priority III: Established Infestations: Weed species in this priority have become established and spread to the extent eradication is not feasible on a Forest. Therefore, noxious weed control efforts are placed on containing the established infestation to prevent spread to uninfested areas. Biological controls are emphasized on main infestations and noxious weed programs are directed at promoting stable plant communities which compete with noxious weeds.

-3-

The above prioritization of noxious weeds for application of efforts is consistent with classical IPM activities.

Additionally, it parallels the goals and objectives of the revised Washington State Noxious Weed Law (EXHIBIT 2), Oregon State's weed classification policy, and the BLM's supplement to the Northwest area noxious weed control program.(EXHIBIT 3). Consequently, adopting the policy and plan in question would aid in providing continuity between state and federal noxious weed control programs.

The Regional plan also provides the basic background information needed for "tiering" project-specific analyses (SEE EXHIBIT 4: Boise National Forest Noxious Weed and Poisonous Plant Control Program Environmental Assessment (April, 1987).)

We believe if weeds are controlled systematically, in the way described above, the Pacific Northwest Region will be able to effectively deal with noxious weeds within the constraints of Federal laws, budget, environmental considerations and resource management objectives.

For the above stated reasons, the final EIS should adopt and incorporate the Intermountain Region noxious weed policy and prioritization plan as set forth in the Record of Decision.

Poisonous Plants and Troublesome Weeds: The BLM appendices cited in the draft EIS (p. III-31, para.2) contain native plant species which are not noxious weeds.

Citizens for Environmental Quality, the Idaho Natural Resources Legal Foundation and the public we represent oppose strongly the use of herbicides on any and all native plant species within the boundaries of Region 6 including but not limited to larkspur (Delphinium spp.), lupine (Lupinus spp.), and milkvetch (Astragalus tweedii).

These native plant species occupy an important niche in the forest ecosystem. We believe that the benefits of native species far outweigh their risk to livestock. As an example, the plants mentioned above provide important essential forage for wild pollinating insects, stabilize the soil, and contain nitrogen fixing bacteria which add to the health and welfare of the ecosystems in which they live. It is important to point out that your agency has a mandate to promote biological diversity. Native plant species -- even poisonous and troublesome species -- promote biological diversity. Just because a plant is poisonous to livestock (foreign invaders themselves) should not be the overriding factor in determining their control.

We also believe that elimination of native plant species may cause invasion by less desirable plants, such as exotic noxious weeds, because the natural niche is disrupted. Therefore, native plant species, poisonous or not, should not be treated with herbicides but left in their native environment to preserve and protect biological diversity.

-4-

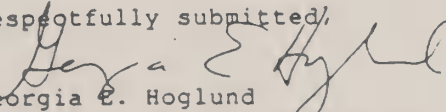
Threatened, Endangered, and Sensitive Species: We officially request that the Idaho Natural Heritage Program be contacted before any herbicide plans on Idaho National Forest System lands within your jurisdiction. (Contact: The Nature Conservancy, Idaho Natural Heritage Program, c/o Idaho Department of Fish and Game, 4696 Overland Road, Suite 518, Boise, ID 8370 (208) 334-3402, (Steve Caicco, botanist/ecologist).)

This professional organization has the only comprehensive data bank on information on the status and distribution of rare, threatened, and endangered flora and fauna in Idaho. A quick initial assessment from the heritage data base will expediate the site-specific environmental review process. Heritage staff planners can also assist Forest Service managers in avoiding or mitigating negative impacts from herbicides, as well as developing management and conservation plans.

We file these comments in good faith. We believe that once the Forest Service begins to develop and implement IPM programs specific to noxious weeds, the national forests will be better managed and the forest ecosystem and communities as a whole will be enhanced.

We reserve the right to submit other comments on the Pacific Northwest Region vegetation management EIS.

Respectfully submitted,


Georgia E. Hoglund
Authorized Representative for
CEQ & INRLF

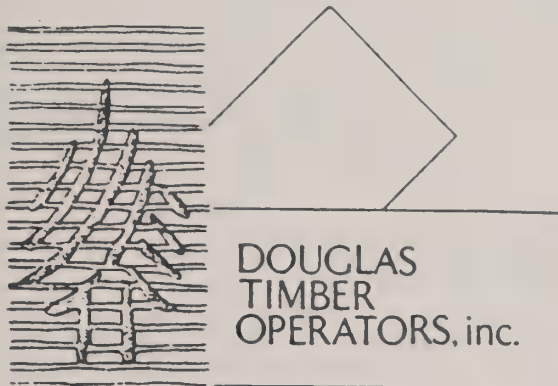
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Idaho Noxious Weed Workgroup
Washington State Noxious Weed Control Board
Oregon Noxious Weed Control Coordinator
Northwest Coalition for Alternative to Pesticides

List of Exhibits

Exhibit	Title	No. pages
1	Record of Decision of ³² the Intermountain Region Noxious Weed & Poisonous Plant Control Program FEIS signed by ³³ Regional Forester, J.S. Tixier on December 15, 1986. (Excerpts) (Entire ROD located in Appendix A of Exhibit 4 as cited below.).....	8
2	Washington State Weed Law , as amended(1987)RCW 17.10.....	35
3	Final Supplement to the Northwest Area Noxious Weed Control Program FEIS, USDI/BLM (Mar.1987) (<u>See</u> page 119).....	151
4	Boise National Forest Noxious Weed & Poisonous Plant Control Program Environmental Assessment. John J. Lavin, Forest Supervisor (April 1987)..	41



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February 4, 1988

Jim Torrence, Regional Forester
USDA-Forest Service, Region Six
PO Box 3623
Portland, OR 97208

Dear Jim:

Douglas Timber Operators, Inc., an association of 74 logging, transportation, manufacturing, and supportive firms in Douglas, Coos, and southern Lane counties would like to take this opportunity to respond to the Vegetation Management DEIS. DTO has examined the document and feels it is severely flawed and inadequate in its analysis.

DTO can not support any alternative that reduces timber supply, jobs, or receipts to counties and schools. If any of these were reduced the economic base of the region would be seriously undermined. For these reasons DTO supports Alternative B-Plus as detailed in the input of the Oregonians for Food and Shelter. This alternative allows for a full range of vegetation management tools, including the use of herbicides for growth enhancement.

DTO's specific comments on the Vegetation Management DEIS are contained below.

HEALTH RISKS ARE NEVER TRULY DISCUSSED IN A QUANTITATIVE MANNER

The DEIS never truly discusses the health risks of various vegetation control methods in a quantitative manner. Instead, the Forest Service chooses to discuss the health risks of herbicides and other vegetation management tools in a qualitative manner. Greater steps need to be taken to put these risks in a solid quantitative framework, and show how these risks compare to smoking, salt consumption, etc..

Page 2
Jim Torrence
February 4, 1988

FLAWED ASSUMPTION OF UNLIMITED BUDGETS

In the analysis of timber growth and yield figures that would result from different levels of vegetation management, the Forest Service assumed an unlimited budget. This resulted in the use of more expensive methods of vegetation control (ie: manual control) being selected over more cost effective methods (ie: herbicide control). It is certainly realistic to make such an assumption in these times of Graham, Rudman, Hollings, and ever reducing budget levels. The Forest Service should take realistic budget constraints and apply them to projected timber yields and produce real life numbers. If such an avenue is taken, DTO is sure you will find that herbicides are, economically and environmentally, the correct choice.

SELECTION OF THREE PREFERRED ALTERNATIVES

The fact that the Forest Service selected three preferred alternatives in the DEIS is extremely confusing to the general public who was asked to respond to the plan. Instead of clarifying the issue and creating an environment where lay people could respond, it created a fog of uncertainty that clouded the process and left some public unable to respond.

DTO hopes that the forest supervisor will eliminate this problem by selecting only one preferred and then asking the public to respond again to the DEIS. The clearly political intent to deflect and dilute public input and criticism is obvious in the decision to select three preferred alternatives.

REDUCTIONS IN TIMBER YIELDS NOT PROPERLY DISPLAYED

The Vegetation Management DEIS does not properly show the effects on timber yield by each alternative. For example, the DEIS shows the reduction in yields under Alternative C to be as little as a 20% reduction in harvest levels. The Umpqua N.F. Timber Staff has shown that the reduction in yields could be as much as 60% or almost 200 million board feet annually. We do not feel the Forest Service has provided reliable information upon which the public can make significant comments at a local level.

Not only is this situation true for Alternative C, but all the projected decreases, or in the case of Alternative G, increases are drastically underestimated. The Forest Service should improve their analysis so as to properly display harvest level effects on a forest by forest level.

Page 3
Jim Torrence
February 4, 1988

UNWORKABLE IMPLEMENTATION PLAN

Contained within the DEIS are several statements that would result in any alternative chosen not to be implementable. For example, the mitigating measures for herbicide use is to "notify all downstream water users". Since the phrase is not defined it is difficult to ascertain exactly what who would have to be notified. It is obvious that any preservationist or obstructionist that wished to halt herbicide spraying could do so easily by simply demanding that all downstream users from the head of the Umpqua to the Pacific Ocean be notified. Without a doubt, an impossible task.

An equally chilling statement in the monitoring plan is the reference that herbicides will not be applied in any water shed. While it may be implied that the watersheds in mind are those specially identified as so by city or county governments, it is never so defined in the document. The concerning effects of this statement could be that if Alternative G was chosen there would be no herbicide use on the Umpqua because every piece of the Umpqua N.F. is either in the North Umpqua, South Umpqua, or Coastal Fork of the Willamette Watersheds. Language such as this is spread throughout the document and should be removed to prevent another court action that would bring us back to ground zero.

NO PEER REVIEW

A peer review of the DEIS was not delegated, completed, or distributed prior to release of the document to the public. It is standard procedure for a scientific document to undergo a peer review prior to its publication. This point was overlooked by the ID Team and has significantly reduced the credibility of the document. DTO and the public would be better able to respond to the DEIS if a peer review was available as a reference source. The complex and technical nature of the document makes it essential that a review be available. Without a review no one but the most educated public can be expected to respond in a thoughtful manner. Therefore, our Congressionally mandated right to public comment has been severely restricted.

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Jim Torrence
February 4, 1988

CONCLUSION

In conclusion, Douglas Timber Operators, Inc., would like to reaffirm its position on the Vegetation Management DEIS.

- * - DTO supports Alternative B-Plus
- * - DTO can not support the loss of jobs, county revenues, school revenues, of allowable sale quantity.
- * - DTO can not support any reduction in the use of herbicides or other vegetation management levels below there pre-injunction levels.
- * - DTO supports the flexibility provided forest managers under the B-Plus Alternative
- * - DTO supports the economic feasibility of the B-Plus Alternative.
- * - We can not support the DEIS because its major concept of basing a technical forestry program on weak subjective data.
- * - We support the use of herbicides for growth enhance on forest plantations in Region Six.

Douglas Timber Operators, Inc., appreciates the opportunity to be involved in the Vegetative Management Development process, and anticipates being heavily involved in the future.

Sincerely,

DOUGLAS TIMBER OPERATORS, INC

A. Troy Reinhart/tcl

A. Troy Reinhart
Executive Director

ATR/tcl

cc: Governor Neil Goldschmidt
Oregon Congressional Delegation
Kate Lilley, Field Representative
DTO Board of Directors
DTO Steering Committee
Oregonian for Food and Shelter
Greg Miller, North West Timber Association
Steve Kadas, Associated Oregon Loggers
Chris West, Northwest Forestry Association
Sue Joerger, Southern Oregon Timber Industries Association



THE DOW CHEMICAL COMPANY

MIDLAND, MICHIGAN 48674

January 11, 1988

FOREST SERVICE

F. Dale Robertson
Chief of The U.S. Forest Service
Forest Service - USDA
P. O. Box 96090
Washington, DC 20090-6090

000640

Attention: Larry L. Gross, USFS, Washington D.C.

Dear Sir:--

This letter is being written at the request of Dr. John Troth, Agricultural Products Department, The Dow Chemical Company, Midland, MI. The purpose of this letter is to describe and summarize the applicator exposure study conducted with GARLON 4 herbicide by Freeman and Yon for Dow Chemical Europe. The importance of this study is that it contains data that can be used to quantify the absorption of triclopyr by individuals applying GARLON 4 herbicide using backpack sprayers similar to those used in the United States. A second method of application involving ultra low volumes of herbicide was also studied but will not be discussed since this method of application is not used in the United States.

In the backpack study, six volunteers sprayed a total of 8 liters of GARLON 4 herbicide, an emulsifiable concentrate containing 480 grams triclopyr acid equivalents per liter. Each volunteer, thus, applied 640 grams (1.4 lbs) of triclopyr acid equivalents which required approximately 4 hours. The herbicide was diluted at the rate of 120 ml GARLON 4 to 20 liters of water and sprayed to the point of run off using Cooper-Pegler CP 3 knapsack sprayers with flat green tip nozzles and operated at 220 kPa. This produced a 3/4 meter spray width. The nominal coverage was 6 liters GARLON 4 per hectare. The volunteers wore a one piece suit, hat, and cotton gloves. Wellington boots were optional. Safety precautions detailed on the label were observed. Battery powered air samplers and denim patches attached to the clothing were used to estimate the potential inhalation and contact exposure while GARLON 4 was being diluted and sprayed. In addition, all urine voided on the day prior to spraying, the day of spraying, and the first 4 post-spraying days was collected and analyzed for triclopyr and creatinine. The amount of creatinine found in the urine specimens indicated the compliance in collecting the urine specimens was excellent. Results of the triclopyr analyses were corrected for recovery based on spiked samples prepared in the field.

* Trademark of The Dow Chemical Company.

F. Dale Robertson

-2-

January 11, 1988

The results of this study are summarized in the accompanying table. The potential exposures estimated from the air samples and denim patches were typical for this type of application. Concentrations of triclopyr in the air were low, ranging from 0.9 to 3.1 ppb. Assuming a respiratory minute ventilation of 1.47 m³/hour, the volunteers inhaled an average of 0.14 mg triclopyr per person during the 4-hour spray period. Analysis of the denim patches indicated that the total body surface contact ranged from 143 to 910 mg triclopyr per person.

The total amount of triclopyr excreted in the urine on the spray day and the first 4 post-spraying days averaged 0.305 mg triclopyr per volunteer. Carmichael et al. (1988) recently found that human volunteers given single oral doses of triclopyr excreted an average of 81.8% of the dose in the urine as triclopyr with a mean half-life of 5 hours. Assuming that the entire oral dose was absorbed, these data indicate that the amount of triclopyr absorbed equals the amount of triclopyr excreted in the urine divided by 0.818. Thus, the backpack sprayers in the study reported by Freeman and Yon absorbed an average of 0.373 mg triclopyr. This represents 0.005 mg triclopyr/kg of body weight or 0.004 mg triclopyr/kg of body weight/pound of triclopyr sprayed. If in a typical day a backpack sprayer applies 3.0 pounds of triclopyr as GARLON 4, these data indicate that an individual will absorb 0.012 mg triclopyr/kg body weight/day.

If you have any questions about these studies or these results please call me.

Cited Studies:

Freeman, J. M. H. and Yon, D. (1983) A study of the exposure of sprayer operators during high and ultra low volume application of Garlon 4 Herbicide in forestry in the United Kingdom. R&D Report, Dow Chemical Company Limited, King's Lynn, England.

Carmichael, N. G., Perkins, J., Nolan, R. J., Fletcher, A. P., Davies, R., Brown, P. M., and Warrington, S. J. (1988) A study of the oral absorption and excretion of triclopyr in human volunteers. Draft R&D Report, Dow Chemical Company Limited, King's Lynn, England.

Sincerely,



Richard J. Nolan, Ph.D., D.A.B.T.
Research Leader, Biotransformation Group
Mammalian & Environmental Toxicology
Research Laboratory
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Phone: 6-2182

cc: R. D. Fears, 9008
A. M. Schumann, 1803
R. J. Sbragia, 9008
J. L. Troth, 9008

RJN:jks

EXPOSURE OF BACKPACK SPRAYER OPERATORS APPLYING GARLON* 4 HERBICIDE IN FORESTRY
IN THE UNITED KINGDOM (SUMMARY OF RESULTS REPORTED BY FREEMAN AND YON, 1983).

Volun- teer	Body Wt (kg)	Conc. of Triclopyr in Air (ppb)	Potential ^a		Total Triclopyr Excreted In Urine (mg)	Amount of Triclopyr Absorbed (mg/kg bwt) lbs triclopyr sprayed ^e		Absorbed ^c Typical Use (mg / kg bwt / day)	
			Inhalation Exposure (mg triclopyr per person)	Contact Exposure (mg triclopyr per person)		(mg) ^d (mg/kg bwt)	(mg/kg bwt)	(mg / kg bwt / day)	(mg / kg bwt / day)
1	54.3	0.9	0.07	819	0.307	0.375	0.0069	0.0049	0.015
2	63.0	2.3	0.17	910	0.307	0.375	0.0060	0.0043	0.013
3	70.0	2.0	0.15	273	0.099	0.121	0.0017	0.0012	0.004
4	76.4	3.1	0.23	156	0.618	0.752	0.0099	0.0071	0.021
5	74.0	1.7	0.12	143	0.139	0.170	0.0023	0.0016	0.005
6	82.0	1.0	0.07	260	0.358	0.438	0.0053	0.0038	0.011
Mean	70.0	1.9	0.14	427	0.305	0.373	0.0054	0.0038	0.012
SD	10.0	0.8	0.06	344	0.185	0.226	0.0030	0.0022	0.006

- a) Calculated assuming the respiratory minute volume was 1.47 m³ per hour during the 4 hour application period.
- b) Amount of triclopyr on denim patches adjusted for ratio of surface area of the denim patches to total body surface area (i.e., 1500 to 20,000 cm²).
- c) Typical use assumed to involve spraying 3 pounds of triclopyr per day.
- d) Amount of triclopyr in the urine divided by 0.818, i.e., the fraction of an oral dose of triclopyr that was excreted unchanged in the urine by human volunteers (Carmichael et al., 1988).
- e) Each participant in this study applied 1.4 pounds of triclopyr acid equivalents.



THE DOW CHEMICAL COMPANY

MIDLAND, MICHIGAN 48674

December 31, 1987

Mr. Gary Larsen
Vegetation Management Group Leader
USDA Forest Service
Pacific Northwest Region
P. O. Box 3623
Portland, Oregon 97208

COMMENTS ON THE PACIFIC NORTHWEST REGION DRAFT EIS

This review will deal only with the issues of risk assessment as a methodology. A separate review will discuss specific toxicological issues.

Interpretation of Exposure Scenarios

Our primary concern with the document is the inconsistency between the application of the risk assessment methods and the interpretation of the results. This inconsistency has arisen because of the misuse of the science of risk assessment.

The fundamental problem is that two exposure scenarios are presented with the implication that one reflects exposure situations which are likely to happen while the other is a reasonable worst case. The labels for the two scenarios ("realistic-routine" and "worst-case routine") as well as the language in the main report (e.g. pIV89, PIV-119) and in the introduction to Appendix D strongly imply that real exposures are being estimated. For example the statement on page 2 of the introduction to Appendix D

"The risk assessment includes analyses of a range of possible exposures -- from realistic to worst case -- resulting from herbicide application. Typical application scenarios (routine-realistic) are used to estimate the doses to workers and to members of the public who may be nearby that may reasonably be expected to occur during routine application" (underlining added)

is in direct contradiction to the qualifiers which appear in Appendix D, but not in the main report. Examples include the following.

"(indeed, given the remote location of most spray areas, it is unlikely that any member of the public will be exposed at all)"

"The probabilities of these accidental scenarios range from unlikely to extremely unlikely. Wherever possible, historical records of accidents were used in determining the probabilities of accident occurrence."

Mr. Gary Larsen
December 31, 1987
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"In addition, no member of the public is likely to receive as high a dose as estimated in this risk assessment;"

"Probability of Estimated Routine-Realistic Public Doses Occurring.
Although these three scenarios represent what can happen under routine operations, the probability of people receiving doses as high as those projected here is quite low."

"A combination of factors makes the possibility of the resident receiving such a dose highly unlikely. First, most treatment areas are located considerably further from any residence than the distance assumed in this analysis -- 600 feet."

It can be seen that the stated purpose of the various scenarios and the actual exposure conditions are inconsistent.

This creates an interpretation problem when the calculated margins of safety (MOS) are compared to standard values of 1, 10 and 100 (see for instance page IV-94) which assume realistic calculations.

The concept of using a realistic worst case scenario along with a more realistic exposure situation is a logical approach to the problem of evaluating safety. It is not scientifically credible, however, to imply a situation is realistic (e.g. routine-realistic) and interpret it as such when, in fact, the scenarios are acknowledged to be highly unlikely.

Calculation of MOS

A further problem with the general methodology in the report is the calculation of MOS's as the ratio of chronic (lifetime) NOEL's and acute exposures. This violates a general concept of risk assessment that the NOEL and the exposure scenario are supposed to be closely matched as to route and duration of exposure. This problem is acknowledged in Appendix D

"This risk assessment uses the MOS approach discussed above in comparing onetime human doses to lifetime animal doses in all of these cases even though this leads to an exaggeration of the risks."

This acknowledgment is made in the Appendix, but the margins of safety are then evaluated relative to standard rules of thumb (1, 100, 1000) in the text. The fact that the margins of safety have been incorrectly calculated invalidates the subsequent interpretation of the results.

Mr. Gary Larsen
December 31, 1987
Page 3

Assumption of Carcinogenicity

A third fundamental problem with the risk assessment is the assumption that all compounds are carcinogenic and mutagenic independent of the data.

Extensive animal and in vitro testing can define, within reasonable limits, whether carcinogenic or mutagenic activity can be expected from a compound. The arbitrary discarding of these data subverts the intended risk analysis and does not allow the reader to differentiate true hazards from arbitrary hazards. The U.S. E.P.A. has a well defined classification system for carcinogenic potential which should be followed in the assessment, and cancer risks should only be calculated where appropriate. An example of the misuse of standard methodology can be seen on page 15, section 3 of Appendix D:

"There is no evidence to conclude that it presents a mutagenic risk to humans. EPA has requested additional picloram mutagenicity studies. The worst case assumption is that it is a mutagen."

Correlation of Carcinogenicity and Mutagenicity

A further general issue is the misinterpretation of the relationship between carcinogenicity and mutagenicity.

The use of the correlation between carcinogenicity and mutagenicity is used in a manner which this reviewer has not previously seen, with the exception of this series of forest management reports. We refer specifically to the qualitative and quantitative prediction of potential mutagenicity inferred from carcinogenicity results.

This is somewhat like inferring that cancer causes smoking. The cause-effect relationship is backwards (see, for instance, Appendix D V-29).

Numerous other examples of misleading conservatism, which defeat the purpose of providing managers, workers, and the public with meaningful estimates of exposure and risk, are present in this report. The more general problem however is the misuse of risk assessment methodologies which runs through the report and other reports in the series of forest management evaluations.

It would appear that the real issue underlying these reports is the prudent use of herbicides. If certain products are not safe to use under the labelled directions their use should be discontinued. If they are safe to use, then applications should be judged in terms of efficacy relative to other alternatives. We certainly would agree that prudent herbicide management practices should be followed and that continuing education and review of work practices are appropriate.

I/B Public Participation and Consultation

Mr. Gary Larsen
December 31, 1987
Page 4

We would hope that these and other comments will be used to constructively review the draft EIS in keeping with methods commonly used by professionals in the field of risk assessment.

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THE DOW CHEMICAL COMPANY

MIDLAND, MICHIGAN 48674

December 31, 1987

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COMMENTS ON THE USFS REGION 6 DRAFT VMEIS

A. Mammalian Toxicology (Mostly based on Appendix D)

As with all recent Environmental Impact Statements, the database upon which the document was written is incomplete. In other words, data are available which have not been used in creation of this document. EPA tox 'One-liners' should not be used as basic documents without further information of some sort and if 'One-liners' are used, these should be compared with publications and other report copies and discrepancies dealt with to resolution.

Triclopyr. Reproductive NOEL

For example, a One-liner for triclopyr cited in Section 3, p. 13 lists the fetotoxic NOEL as <10 mg/kg/day. The original report and its subsequent publication show the NOEL to be >25 mg/kg/day which was the highest dose tested. The data support the NOEL as >25 mg/kg/day because no adverse effects occurred to the offspring in this rabbit study at either dose, 10 or 25 mg/kg/day. See Hanley, et al, Fund. & Appl. Toxicol., 4, 872-882, 1984. (Attachment 1)

Triclopyr Systemic NOEL

Table IV-19 on p. IV-101 of the EIS lists NOEL's for all of the herbicides. The triclopyr systemic NOEL is shown as 0.5 mg/kg/day with reference to liver clearance, and liver weight effects at higher doses. This is incorrect. The systemic NOEL for triclopyr was correctly stated in the Appendix as 2.5 mg/kg/day. Kidney effects occurred in dogs (the most sensitive species) at higher doses.

Data Gaps

The identification of 'data gaps' in Section 1, Appendix D, p.9 and Table 1-2, p.9a is misleading. The table does not clearly point out what the x's indicate; also, the lack of a specific report to fill a mechanically-derived slot does not mean that such data would be relevant for the material in question nor that the data have not been provided through other means (such as in another report which shows low toxicity

Mr. Gary Larsen
December 31, 1987
Page 2

or hazard and thus no need for such a test). Note: triclopyr does not appear in Table 1-2--this must be an oversight.

As an example of the problems associated with creating data gap listings, Table IV-21, p. IV-103 of the EIS notes that there are no neurotoxicity data for triclopyr or picloram. This is most certainly not the case since just about all of the mammalian tests on these compounds include careful evaluations of behavior during the study as well as gross and microscopic examinations of the central and peripheral nervous systems. Because of the negative findings in these tests, special tests for neurotoxicity need not be conducted for either registration or purely scientific reasons.

Section 3, pp.12-13 contains statements which indicate that additional chronic/cancer studies are needed and have been requested for both picloram and triclopyr. These studies have been completed, both are negative for cancer, and results are available to the authors.

Picloram and Cancer Risk

A cancer risk for picloram has been calculated based on the NTP study results which showed an increase in neoplastic nodules in the livers of female rats ingesting large doses of picloram for life. (Neoplastic nodules are benign lesions which did not progress to cancer/malignancies). Note: Section 3, p.23 should at least read 'benign liver nodules' instead of 'liver tumors'. The newest picloram rat study, which was obviously not available to the writers, shows no indication of neoplastic nodules or of any other tumorigenic response. This latest rat study was conducted in order to finally answer the question about picloram's possible carcinogenicity---and it did---picloram is not carcinogenic and no cancer risk should be calculated for it. We request that discussions throughout the risk assessment clearly indicate that malignant tumors have not been observed in chronic studies with picloram and that interpretation of the benign liver effects in the female rat as an indicator of carcinogenicity is subject to scientific controversy, particularly since the liver is the target organ for systemic effects of picloram. (See new publication: Gorzinski, et al, J. Tox. Env. Hlth., 20, 367-377, 1987.) (Attachment 2)

Carcinogenicity/Mutagenicity Generally

On p.10 of Section 1, Appendix D, the statement "The probability of mutagenic activity was based on available cancer data" is a misuse of scientific data. To judge mutagenic potential from the results of cancer studies is scientifically backwards; mutagenic events are one of several potential causes of carcinogenic response, not vice versa. The whole idea behind studying chemicals for mutagenic effects is to predict their carcinogenicity or their effects on the unborn and so chemicals

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December 31, 1987
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which have definitive tests for cancer and birth defects do not need an additional mutagenicity assessment. Picloram and triclopyr are clearly not mutagenic although Section 3, pp. 16-17 show that they are assumed such. 2,4-D may have some weak mutagenic potential in certain systems but it, too, should not be broadly described as mutagenic as shown on p. 15, Sec.3. If scientific logic is not employed in assessing carcinogenic potential, nearly all chemicals could eventually be labeled as carcinogens and the end result is that reviews such as the EIS would become meaningless since true carcinogens could not be differentiated.

2.4-D Neuropathy

Although certain publications implicate 2,4-D as the causative agent in a few cases of human neuropathy, a thorough examination of these publications and results of animal testing show that there is no reason to draw a cause and effect conclusion between 2,4-D exposure and human neuropathy. Mattsson, et al (Fund. & Appl. Toxicol., 6, 175-181, 1986) state, "One of the criteria for establishing a causal relationship between chemical exposure and neurotoxicity is that the condition be reproducible in animals". They found their results "consistent with other studies () that found no neuropathologic consequences of repeated exposure to 2,4-D in rabbits, rats, pigs, chickens, or dogs". Thus, statements such as, 'the herbicide (2,4-D) has been reported to produce peripheral neuropathy in a few individuals after accidental exposure' (Appendix D, Section 3, p.9), should be modified to include the improbability of this as an effect of 2,4-D. (Attachment 3)

2.4-D References

The listings of 2,4-D references in Appendix H, pp. 16 and 29 are incomplete. They do not include the many recent Industry Task Force Studies which are available at this time.

Synergism

In Section 3, p. 31 there is a statement concerning so-called evidence of synergism between 2,4-D and picloram arising from a skin sensitization test. This study was one of many conducted on various 2,4-D/picloram mixtures and the only one in which any 'positives' were seen. There are many other possible reasons for the couple of positive human responses in this test such as unusual sample composition, misleading of results, previously sensitized test population, etc., but synergism is only a remote one--one without any credibility when one examines all of the other sensitization and metabolism information for these two materials.

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B. Exposure Analysis Data & Assumptions

Since most of the herbicides reviewed in the EIS have not been tested on humans for metabolic fate, pharmacokinetics, or dermal absorption, data for 2,4-D taken from the work of Lavy and others were used in exposure calculations. The figure generally used for dermal penetration of herbicides without individual data is 10% -- see p.9, Section 4. New data, now available for inclusion in the revision, show that triclopyr has a dermal penetration rate of 1.65 percent with a standard deviation among human volunteers of 1.00 percent (Attachment 4). This value should be incorporated into all risk and exposure calculations for triclopyr. The effect is to reduce all estimates of exposure by more than 6-fold and to also increase the corresponding margins of safety (the ratio of the NOEL to exposure) by the same more than 6-fold value. Then, for example, the estimated exposure for a routine-realistic scenario for a backpack applicator would become 0.06 mg/kg instead of 0.33.

Using some example calculations supplied by LAI from their work on the Region 8 Risk Assessment, the exposure for a backpack applicator then changes as follows: (assumptions may vary from Region 6 EIS) (Attachment 5)

- acres treated/day=3
- protective clothing factor=0.321
- average dose from Lavy, 1984=0.03297 mg/kg/lb applied
- factor for new dermal penetration rate=0.275 (i.e., 1.65/6)

Total pounds applied = 1.4 lb/ac x 3 ac/day = 4.2 lb/day

Dose = 0.321 * 4.2 lb/day * 0.03297 mg/kg/lb * 0.275 = 0.012 mg/kg/day

This dermal penetration rate should also be used in wildlife exposure calculations and in predictions of human dose from ingestion of game.

Accidental Over-spraying

Additional information concerning accidental over-spraying of a human may be obtained in a paper by Frank, et al, Arch. Env. Contam. & Toxicol. 14, 427-435, 1985. The accidentally oversprayed individual was wearing shorts, a T-shirt (bare arms), and sneakers. This paper and calculations for an analogous accidental exposure to triclopyr at 4 (typical) and 8 (maximum) lb/ac use rates have previously been supplied to LaBat-Anderson and should be used in this and all other related EIS's. Margins of safety indicated by data from this paper are many-fold greater than those in the risk assessment. (Attachment 6)

Mr. Gary Larsen
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Brown and Burn

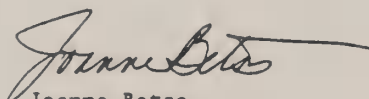
In the section on burning, Appendix D, Section 5, p. 11, the assumption (#6) that all herbicide residue is released to the atmosphere upon burning is not correct. Data on many of these herbicides, including picloram and triclopyr indicate that most of the herbicide is destroyed during burning and very little is released to the atmosphere even under non-ideal conditions. Bush, et al (Arch. Environ. Contam. Toxicol., 16, 333-341, 1987) state that the percent carried over from smoke was less than 0.1% for both picloram and triclopyr under fast combustion rates. It should also be noted that both intentional fires (for clearing) and accidental fires occur under conditions which favor fast combustion. Utilization of the data developed by Bush, et al would provide both forest workers and the public with a much more reliable estimate of exposure which may result from burning of treated vegetation. (Attachment 7)

C. General Comments

This EIS is entirely too long! It is overly wordy and repetitive. It reads as if it were written to mean 'all things to all people'. A more concise, directed approach would be much more effective.

Table 3-3 needs herbicide names to be added.

Table 5-16 uses the name Dicloram, should be Picloram.



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hlb

attachments (6)

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THE DOW CHEMICAL COMPANY

MIDLAND, MICHIGAN 48674

October 30, 1987

John Troth
9008 Building
Ag Products Department

cc: Robert Fears, 9008 Bldg.

PHARMACOKINETIC STUDIES ON TRICLOPYR AND GARLON - DOW EUROPE

This letter is to summarize the results of the recently completed pharmacokinetic studies conducted on triclopyr and GARLON* in human volunteers by Dow Europe. The reports for these studies have not yet been written, but I am able to summarize the results of these studies because I was asked to analyze these data.

In the first study, six male volunteers were given single oral doses of 0.5 and 0.1 mg triclopyr/kg of body weight. The same volunteers participated in both segments of this study and there was a 3 week interval between the 0.5 and 0.1 mg/kg dose. The triclopyr was dissolved in apple juice and ingested following an overnight fast. No untoward effects were reported as a result of these doses. Following ingestion of the triclopyr, the volunteers collected all urine voided during the next 72-hours. Blood specimens were also collected at specified intervals. These urine and blood specimens were analyzed for triclopyr. In 72 hours these volunteers excreted $80.1 \pm 13.0\%$ of the 0.5 mg/kg and $83.5 \pm 9.8\%$ of the 0.1 mg/kg oral dose in their urine as unchanged triclopyr. The average half-life for the elimination of triclopyr was 5 hours.

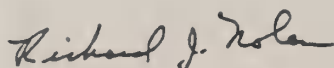
In the second study, five male volunteers (same individuals who participated in the oral study) were given a single dermal dose equivalent to 3.5 mg triclopyr/kg which was applied to the flexor surface₂ of the forearm as GARLON 4 (estimated rate was 10 μ l of GARLON 4/cm²). The dosed area was kept dry and left uncovered for 8 hr. After 8 hr the residue was removed by rubbing the dosed site with a paper towel. Blood and urine specimens were collected and analyzed for triclopyr as in the oral study. The time course of triclopyr in the volunteers following dermal administration was well described by the same pharmacokinetic constants and model used to describe the time course of orally administered triclopyr. The half-life for the absorption of dermally administered triclopyr was 16.3 hr. In 96 hr, $1.39 \pm 0.81\%$ of the triclopyr applied to the forearm was excreted unchanged in the urine. Based on

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John Troth
October 30, 1987
Page Two

the concentrations and amounts of triclopyr found in the blood and urine, these volunteers absorbed $1.65 \pm 1.00\%$ of the triclopyr applied to the forearm; this calculation includes corrections for both the incomplete urinary excretion of triclopyr following oral administration and the excretion of small amounts of triclopyr after the last urine specimen was collected.



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bdc

Outline of Worker Exposures for Region 8 Risk Assessment

All of the worker exposures are based on data from field studies of 2,4-D applicators. Doses were corrected for differences in dermal penetration rates (DPR) of the herbicides relative to the DPR of 2,4-D. A DPR of .6 percent was assumed for 2,4-D (Feldman and Maibach, 1974). No DPR for triclopyr is available in the literature, so a DPR of 10 percent was assumed for triclopyr based on studies in the literature with a number of pesticides. Therefore, for triclopyr a correction factor of ~~1.667~~ was used.

0.275

Aerial

Doses for workers involved in aerial applications are based on the following average doses from worker studies with 2,4-D:

Pilot	= 7.55×10^{-5}	mg/kg/lb applied	Lavy et al., 1982; Franklin et al., 1982; and Nash et al., 1982
Batchman	= 1.08×10^{-4}	mg/kg/lb applied	same sources as above
Observer	= 2.45×10^{-6}	mg/kg/lb applied	Lavy et al., 1982

For typical exposures, the typical application rate and acres treated/day were used. Protective clothing factors were used as follows: .419 for pilots and .729 for batchmen.

Maximum exposures were calculated using maximum application rates and maximum acres treated up to 160 acres/day. It was assumed that no protective clothing was worn.

Example calculation for triclopyr ester - typical pilot:

Total pounds applied = 4 lb/ac * 50 acres/day = 200 lbs/day
Dose = .419 * 200 lb/day * 7.55×10^{-5} mg/kg/lb * ~~1.667~~ = ~~.0205~~ mg/kg/day

0.275 0.0018

Mechanical Ground

Doses for workers involved in mechanical ground applications are based on average doses in Nash et al., 1982 for the three worker categories as follows:

Applicator = 1.7×10^{-4} mg/kg/lb applied
 M/L = 1.75×10^{-4} mg/kg/lb applied
 A/M/L = 2.4×10^{-4} mg/kg/lb applied

For typical doses the following protective clothing factors were applied:

Applicator = .312
 M/L = .729
 A/M/L = .5205 (average of the two above)

Typical application rates and acreages treated per day were used. For maximum exposures it was assumed that no protective clothing was worn. Maximum rates and acres treated were used.

Example calculation for triclopyr amine - typical applicator:

Total pounds applied = 4 lb/ac * 100 acres/day = 400 lb/day
 Dose = $.312 * 400 \text{ lb/day} * 1.7 \times 10^{-4} \text{ mg/kg/lb} * 1.667 = .035 \text{ mg/kg/day}$
~~0.275~~ ~~0.006~~

Backpack

Doses for backpack sprayers are based on average doses from Lavy et al., 1984. The average dose from the study of .07564 mg/kg is equivalent to .03297 mg/kg/lb applied.

This was derived from 62 acres treated by 20 workers at 0.74 lbs/acre, which equals 2.294 lbs applied/worker. For typical doses a protective clothing factor of ~~0.312~~ was used. The typical application rate was also used. Based on Lavy et al., 1984, it was assumed that workers treated

Example calculation for triclopyr amine - typical backpack:

0.275 0.012

Doses to workers using basal bark/stem, basal soil spot, and cut surface treatment methods were based on the exposures to workers using the hack and squirt method in Lavy et al., 1984. Workers in the Lavy et al. study applied 2,4-D at a rate of 1 lb/gallon. In this study, the arithmetic mean exposure was .0285 mg/kg; however, the data set was not normally distributed and was negatively skewed. The data was logarithmically transformed in order to obtain a better estimate of the mean. From the transformed data a mean of .015657 mg/kg was derived. This dose was adjusted based on 6 hours of application time to .0026 mg/kg/hr. Doses for Region 8 workers were calculated based on the concentration (lb/gal) of the herbicide, its dilution in a tank mixture, and the number of hours worked per day. Typical doses used a protective clothing factor of 0.424 for hack and squirt workers. Typical doses used typical hours worked per day. Maximum doses used maximum hours worked per day and no protective clothing.

0.275 0.003

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E. I. DU PONT DE NEMOURS & COMPANY

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AGRICULTURAL PRODUCTS DEPARTMENT

February 11, 1988

Mr. Gary Larsen
Vegetation Management Group Leader
USDA Forest Service
Pacific Northwest Region
P. O. Box 3623
Portland, OR 97208

Dear Mr. Larsen:

Subject: Draft Environmental Impact Statement (DEIS) --
Managing Competing and Unwanted Vegetation

The Du Pont Company welcomes your efforts to produce an environmental impact statement that properly weighs the benefits and risks of herbicide use in the Pacific Northwest Region. I thank you for this opportunity to review and comment on the subject DEIS. As a toxicologist in Du Pont's Agricultural Products Department, I have experience in the conduct and interpretation of toxicity tests and with utilizing these test data to assess the safety of our products. Based on my review of this document, I have attached specific comments regarding the safety data presented for our products (Attachment 1) and more generic comments regarding the exposure and risk assessments based on these data (Attachment 2).

Du Pont realizes the potential value of this document as a planning tool for your vegetation management programs. To be of value, the EIS must accurately reflect currently available information and procedures utilized to interpret and process this information. I offer the following as general comments and areas where improvements are needed:

1. The DEIS relies too heavily on brief EPA data summaries. These are often outdated and do not allow an opportunity for your staff to independently review and interpret the data. Assessments of data quality and adequacy are based on reviews of these summaries rather than on the full studies.
2. The authors assume that all oncogenicity data in laboratory animals should be equally weighted for human risk assessments. This does not reflect current thinking of the scientific community.

BETTER THINGS FOR BETTER LIVING

-2-

3. Quantitative risk assessment models greatly overestimate the cancer risks for man. The One-Hit model used in this document is one of the most conservative. When used along with conservative/exaggerated exposure estimates, the overestimation of oncogenic risks for man will be even greater.
4. Current EPA guidelines suggest that the use of quantitative risk assessment is inappropriate for weak oncogenic responses. For example, the mouse liver tumor response in bromacil studies. The guidelines indicate that a margin-of-safety approach is more appropriate for bromacil hazard assessments.
5. The exposures and health risks for the "routine realistic" and "routine worst-case" scenarios are based on several conservative assumptions that compound overestimates of potential health hazards. These assumptions should be revisited. The "routine realistic" scenarios in many instances should be defined as the "routine worst-case" scenario.
6. The Appendices provide caveats and some discussions of the conservative assumptions used in the document. Summary tables do not contain references to these caveats. The danger is that the summary data will be interpreted as being absolute and may be misused by those who do not read the full document.

Of the vegetation management options presented in this document, Du Pont supports Option "B", which makes all tools available to those charged with managing our forestlands. The available products can be used without unacceptable risk to man and the environment. Newer safer products, safer formulations and application methods for existing products are continually being developed. We feel that Option "B" does not present unacceptable risks. Through continued training programs and new product developments, these risks will be further reduced.

If you or your staff have specific questions regarding this information, or if I may be of assistance, please call me at (302) 992-6270.

Respectfully,



Fredrick O. O'Neal, Ph.D.
Registration Toxicologist
Registration and Regulatory Affairs

FOO:lah 43(6)

cc: Dr. Jim Witt, Oregon State University

Attachment 1
E. I. du Pont de Nemours & Co., Inc.
February 11, 1988

PRODUCT SPECIFIC COMMENTS FOR THE
PACIFIC NORTHWEST REGION DEIS

<u>ACTIVE INGREDIENT</u>	<u>SECTION/ PAGE</u>	<u>COMMENTS</u>
Bromacil	D-3/76	NOEL should read "greater than 250 ppm (7.5 mg/kg/day HDT) rabbit teratology study."
Bromacil	D-3/20	There was no discussion or qualification of the evidence of carcinogenicity for bromacil. Current EPA Risk Assessment Guidelines (EPA, 1986) would classify these mouse liver tumor data as only "limited" evidence of carcinogenicity, a "Group C" oncogen. As such, the quantitative risk assessment estimate of cancer potency, for example, on pages D-3/26 and D-5/4 are considered inappropriate.
Bromacil	D-/27b	The half-life for foliar dissipation and degradation of dislodgeable bromacil residues was excessive (60 days) and resulted in greatly exaggerated exposure estimates (Table 4-9).
Bromacil	D-5/28&29	Page 28 includes the statement ". . .increased risk of cancer can be used to approximate the quantitative risk of heritable mutations." Page 29 suggests mutagenic risks for bromacil can thus be estimated. The logic behind these statements is not based on current understanding of cancer induction. Not all carcinogens are mutagens. Mechanisms other than direct genetic damage are associated with some cancers. The mouse liver tumors produced in the bromacil study are an example. The weight of evidence indicate that bromacil is non-mutagenic.
Bromacil	D-A/2	The statement "no validated mutagenicity studies have been completed," for bromacil is inaccurate. Attachment 3 summarizes the bromacil mutagenicity data. The weight of evidence from numerous <u>in vivo</u> and <u>in vitro</u> tests indicate that bromacil is neither mutagenic nor genotoxic.

Attachment 1 (cont'd)
 E. I. du Pont de Nemours & Co., Inc.
 February 11, 1988

ACTIVE INGREDIENT	SECTION/ PAGE	COMMENTS
Diuron	IV/101	The NOEL was based on lowered red blood cell count; Table IV-19.
Diuron	D-3/11	Systemic NOEL should be 250 ppm (12.5 mg/kg/day; Hodge, et. al., 1967).
Diuron	D-3/15	The statement "there are no validated mutagenic studies on diuron," is inaccurate. Four studies were completed in 1984/1985 and later submitted in support of diuron. Negative results were obtained in three of four tests: the Ames Test; point mutation assay in CHO cells; and a DNA repair assay with rat liver cells. Only one of four tests was weakly positive, an <u>in vivo</u> cytogenetics test. The weight of these data suggests that diuron is not mutagenic. Page D-5/29, Table 3-4 and other references to the absence of diuron mutagenicity data should be amended.
Diuron	D-5/16	Routine-Realistic Scenario The statement that backpack sprayers "are exposed to a diuron dose that exceeds the systemic NOEL," is misleading. Due to compounding conservative assumptions, the Routine-Realistic exposure estimates are exaggerated. Therefore, the margins of safety reported for diuron and other products are correspondingly underestimated. The years of Du Pont experience with diuron applications via backpack indicate no pattern of illness or unacceptable risk among workers.
Diuron/ Fosamine	IV/100 & IV/116	Code of Federal Regulations, Vol. 40, Part 158, contains toxicology studies required for safety evaluations of products used for non-food crop of forestry applications. Acute, subchronic, and special studies to test for mutagenicity, are required. The toxicity databases for diuron and fosamine ammonium exceed these minimum requirements. This point should also be included with references to "missing information" for these products.
Fosamine	D-3/22	There are no 2-year feeding studies with fosamine. Delete the last sentence referencing this compound.

Attachment 2
E. I. du Pont de Nemours & Co., Inc.
February 11, 1988

GENERIC COMMENTS FOR THE
PACIFIC NORTHWEST REGION DEIS

SECTION/ PAGE	COMMENTS
IV/105	Table IV-23 summarizes the quality of data that exist for the 16 active ingredients. Since much of the data was obtained from EPA in summary form (e.g., Tox one-liners), the preparers could not have made the detailed, independent review of full studies required to adequately assess study quality. This table is misleading in that it implies a thorough review by the writers.
IV/105	Table IV-23 indicates that few products have Immunologic and Neurologic test data. This, too, is misleading since evaluation of these parameters is incorporated to some degree in acute, subchronic and chronic toxicity tests. The pesticide industry has little guidance as to immuno- and neurotoxicity test requirements. Tests to specifically assess these effects are still under development.
IV-104	Table IV-22 implies that the cell transformation assay data is necessary to assess mutagenicity. EPA only requires representative tests in 3 general areas. This requirement may be filled without the cell transformation assay.
IV-117	Impurities. The statement "Most tests utilize pure active ingredients . . ." is not accurate. EPA requires that the most extensive testing be conducted with technical grade or manufacturing use product and not pure product. Therefore, the risk of a potent dioxin-like, highly toxic contaminant is unlikely. The references to toxic contaminants should be deleted.
D-1/1	Safety factors for inter-and intraspecies variability has been discussed with reference to hazard (margin-of-safety, MOS) assessments and extrapolations to man. These MOS values are actually larger when one considers the differences between the "threshold-" and "no-effect" levels in an animal study (Fig. 3-1). This difference is variable, is related to the selection of doses at the beginning of the test, and can be equated with an additional MOS which is usually not quantitated. This fact should be considered when comparing MOS's for various products.

Attachment 2 (cont'd)
E. I. du Pont de Nemours & Co., Inc.
February 11, 1988

<u>SECTION/ PAGE</u>	<u>COMMENTS</u>
D-1/9 & 10	Costs for conducting toxicity tests are much higher than prices typically experienced by industrial or contract labs.
D-3/18	Table 3-4 summarizes oncogenicity data for the 16 products. Like other references to oncogenicity of these products, there is no attempt to consider the severity of the tumor effect in animals nor to consider the relevance of these effects to man. The assessment of risk to man cannot be done adequately without these considerations. The authors treat all tumors produced in laboratory animals with the same level of concern for the oncogenicity risk assessments. This is not in accordance with modern oncogenicity assessment practices (EPA, 1986).
D-4/6-10	Routine Realistic Exposure Scenario. These represent exposure overestimates for one or more of the following reasons: no protective clothing was worn by workers in the reference study used for exposure assessment; decline of total rather than dislodgeable foliar residues were used for worker exposure; total area treated is greater than that currently experienced by forestry workers; and the proximity of the public to the treated areas was closer than typically experienced. When compounded with other conservative assumptions, the routine realistic risk for each exposure scenario will be consistently exaggerated. The realistic exposures and risks will therefore be lower than indicated by this DEIS.
D-5/4	Quantitative risk assessments for oncogenic effects routinely overestimate or exaggerate the risks at low-dose exposures (Food Safety Council, 1980). The One-Hit model used for the assessments is the least flexible and produces the highest estimates of risk. The biological rationale used to develop this model is not consistent with mechanisms of carcinogenicity associated with herbicides in this document. The model assumes the products are genotoxic. This is not the case for bromacil is not likely true for other products.

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Chapter 11. In Proposed System for Food Safety
Assessment.

ATTACHMENT 3

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ESTABLISHED 1802

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CENTRAL RESEARCH AND DEVELOPMENT DEPARTMENT

November 6, 1986

JOHN C. SUMMERS
AGRICULTURAL PRODUCTS DEPARTMENT
E-402/3220

REVIEW OF BROMACIL MUTAGENICITY
(RE: YOUR MEMO OF 8/12/86 TO AMS)

Based on the weight of negative evidence from our review of the data, we conclude that bromacil does not pose a mutagenic hazard to humans.

Generally, mutagenicity risk assessment is determined in experimental systems by measuring effects on two primary genetic endpoints: chromosomal aberrations (CA) and gene mutations [1]. Greater weight is placed on tests conducted in germ cells than in somatic cells, on tests performed in vivo rather than in vitro, in eukaryotes rather than prokaryotes, and in mammals rather than in submammalian species. Studies on DNA binding and repair are considered to be supportive and may indicate the mechanism whereby a material causes gene mutations or chromosomal aberrations.

CHROMOSOMAL ABERRATION ENDPOINT

- Bromacil was negative in the following in vivo mammalian CA tests:
 - mouse micronucleus (somatic cells) [2,3], and
 - rat dominant lethal (germ cells) [4-6].
- Bromacil was positive for inducing chromosomal damage in barley [7,8]. However, the significance of chromosomal damage in plants to human risk assessment is unknown.

These results indicate that bromacil does not pose a hazard to man as evaluated by this genetic endpoint.

GENE MUTATION ENDPOINT

- Bromacil was negative in the following in vitro gene mutation tests:
 - Salmonella typhimurium/Ames [2,3,6,9,10,11,13],
 - Escherichia coli [2,3,6], and
 - Saccharomyces cerevisiae mitotic gene conversion [2,3,6].

JOHN C. SUMMERS

- 2 -

November 6, 1986

- Bromacil was positive in vitro in the mouse lymphoma L5178Y/TK⁺/⁻ forward mutation assay [2,3]. These results are not considered to be significant for assessment of human risk in view of the overwhelmingly negative results in other gene mutation assays and because bromacil does not interact with DNA or induce DNA damage and repair in vitro or in vivo as discussed below. While positive results in the mouse lymphoma assay can be due to chromosomal damage [21], no such damage was observed in vivo in mammals. Because bromacil does not interact with DNA or chromosomes in vivo, we postulate that bromacil's structural similarity to trifluorothymidylate, the selective agent used in this assay, may have contributed to the "false positive" results.
- Bromacil was positive in vivo in 2 of 6 Drosophila sex-linked recessive lethal (insect) assays [2,3,16-20]. Four of the studies do not provide sufficient experimental details to evaluate their significance. Results in one study are significant only if multiples are included in the statistical analysis, and are negative in another. As discussed above, there is no supportive in vivo mammalian evidence for bromacil interacting with DNA. We believe that toxic or repellent effects, which are commonly observed in this assay, may have been involved.

DNA DAMAGE AND REPAIR ENDPOINT

- Bromacil was negative in vitro in the following tests:
 - Salmonella typhimurium differential toxicity [2,6,13],
 - Escherichia coli thymidine replacement assay [11],
 - Bacillus subtilis rec assay [2,6,13],
 - sister chromatid exchange in Chinese hamster ovary cells [2,3,6], and
 - unscheduled DNA synthesis in human fibroblasts [3,6].
- Bromacil also was negative in vivo for DNA binding and/or incorporation in mice [15].

The above in vitro and in vivo results indicate bromacil does not interact with or cause damage to DNA in biological systems.

CONCLUSIONS

- Since no in vivo chromosomal damage was observed in mammalian somatic and germ cells, we conclude that man is not at risk for chromosomal damage as evaluated by this genetic endpoint.
- Positive results in the mouse lymphoma and Drosophila assays are not supported by the in vitro and in vivo mammalian DNA binding and repair studies, all of which were negative. Therefore, we conclude that man is not at risk for gene mutations as evaluated by this genetic endpoint.

The studies cited in this memo were reviewed by G.T. Arce, R.G. Stahl, Jr., R.E. Staples, D.R. Vincent and D.A. Vlachos.

Ralph G. Stahl, Jr.

RALPH G. STAHL, JR.

RGS/11

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001313

February 15, 1988

To: Gary Larsen, Vegetation Management Group Leader
USDA Forest Service, Pacific Northwest Region
P.O. Box 3623, Portland OR 97208

From: Robert Brothers, Earth First! Siskiyou
Box 212, Williams OR 97544

Subject: COMMENTS on the DRAFT ENVIRONMENTAL IMPACT STATEMENT
for "MANAGING COMPETING AND UNWANTED VEGETATION".

These documents are insultingly incomplete, and you would do best to retract this Draft EIS.

Normally, I like to be polite, but we're talking about people's lives here, and the poisoning of natural ecosystems.

That secret Dow report uncovered by the court case in Texas (see Attachment #8) is eventually going to get 2,4-D taken out of the Forest Service arsenal. With further testing and more secret reports, it will become unacceptable to use any chemical poisons at all.

What the Forest Service should be doing now is what it should have been doing for the last four years since herbicides were banned: working aggressively to develop alternative methods of mitigation for the elimination of native vegetation by clearcutting ... or better yet, using other harvest methods (natural selection cutting plus natural seeding) that do not require such expensive efforts to restore what has been destroyed.

The bulk of these Comments will focus on the alleged needs for herbicides and vegetation management, because of their relevance to larger issues of national forest management. Earth First! Siskiyou's Comments on the Siskiyou NF's Forest Plan stressed the need for a separate EIS dealing with a full range of alternative practices from timber harvest through site preparation, conifer regeneration, release, and thinning. If such a study were completed for every Forest, then a Region-wide EIS would only be needed for herbicide toxicity.

Evaluation of needs and efficacy of treatments should, under ideal circumstances, be the primary concern of the Forest Service with regards to vegetation management. Evaluation of the health effects of toxic chemicals on humans and other animals and plants is obviously outside the range of USFS expertise. Therefore, the proposed use of toxic chemicals poses a great problem for the Forest Service, and this problem is not adequately resolved by the DEIS.

The information presented in the current document, along with the crucial information that is not available, leads this reviewer to the obvious conclusion that some people and some critters are going to die if herbicide use is resumed in the national forests. This is a straightforward deduction from various studies (such as that of the Kansas farmers) which show increased death

rates from cancer after exposure to herbicides. If people die, so do those other mammals known as "wildlife". Unfortunately, these facts are not portrayed clearly enough in the so-called worst-case analysis in the DEIS. For this and many other reasons already made clear to you by NCAP and other organizations, the worst-case analysis is not adequate enough to be supported by judicial review. For this reason alone, the DEIS should be withdrawn.

There's a paradox here. If you're uncertain about the health effects of herbicides, how can you be certain about choosing a preferred alternative which will employ them? Don't try to whitewash this issue. Uncertainty about risks should not be presented in a way that allows for misunderstanding. We already know that the hazard from these chemicals is great enough to kill people, because people have already died from exposure to them. By proposing the use of known killers, the Forest Service is stating that economic factors can somehow justify loss of life. Don't hide this basic assumption!

The use of known carcinogens amounts to premeditated and indiscriminate random murder. Exactly who will die is not known, but that someone will die is certain.

The crushing irony here is that the Forest Service does not even have the evidence to prove that the use of herbicides is even necessary, under any circumstances, for the maintenance of current levels of timber harvest, revenues and jobs.

CERTAIN RISKS, UNCERTAIN BENEFITS:

"Better Data Needed to Determine the Extent to Which Herbicides Should be Used on National Forest Lands"

This is the title of a report issued by the General Accounting Office in April of 1981, as you are well aware. It echoed the demands which forest workers had been making for the previous four years regarding the need to collect information on the effectiveness of herbicides (see Attachment #1).

If the Forest Service had chosen to promptly and fully comply with the National Forest Management Act (NFMA) of 1976, then we would have over ten year's worth of information with which to evaluate this question. Section 4 (d)(1) of the Act (see Attachment #2) specifies that:

"All national forest lands treated from year to year shall be examined after the first and third growing seasons and certified by the Secretary ... as to stocking rate, growth rate in relation to potential and other pertinent measures."

If such reports had been compiled and analyzed, then correlations of herbicide release, non-herbicide release, and non-release with conifer stocking and growth in the plantations could be evaluated. However, this data has neither been fully gathered nor analyzed.

For example, on the Siskiyou NF, while Plantation Survival Reports on stocking levels are available from 1975 forward, growth rate data have only been presented for the last two years, and no analysis has been done. [See Attachment #3 for a brief analysis of these reports compiled by EF! Siskiyou.]

The absence of this crucial information is acknowledged in various places in the DEIS (Appendix A), but nowhere is it given sufficient emphasis.

The few research studies cited in the document are able to describe only 55% of the commercial forest land base intensively managed for timber production (p.A-9). For information regarding the remaining 45% (3.6 million acres) you were forced to rely only on information specially requested from Forest Silviculturists. If the reports submitted by the Siskiyou NF are indicative of the general quality of reports from all Forests, then the information contained therein could be most charitably described as "anecdotal". Garbage in, garbage out.

Thus, it seems that your information regarding the need for herbicides is even less complete than the information regarding their health effects. Only two out of sixteen chemicals had no health effects data, whereas the needs of almost half of the commercial forest lands for herbicides remains unknown.

On this basis, any Vegetation Management DEIS put out at this time would have to be characterized as "premature"

DISCREPANCIES IN THE PREDICTED EFFECTS OF NO HERBICIDE USE:

- 1979 Siskiyou NF Timber Management Resource Plan, p.iv:
Reduction in 1st decade harvest due to lack of herbicides = 38%
- 1982 Siskiyou NF Forest Plan DEIS, p.44:
Reduction in 1st decade harvest due to lack of herbicides = 17%

Strangely enough, so far as I can tell this question was not directly answered by the Siskiyou NF in reports submitted to the VM EIS team. However, the report dated 10/24/86 stated that 38.6% of the acres needing site prep and release were evaluated as having no acceptable nonchemical treatment. In the more comprehensive report of 1/9/87, the SNF mentioned unspecified amounts of yield reduction due to no herbicides for site prep under Alternative A, and that 16% of identified release needs are unsuited for nonchemical methods, while the effectiveness of nonchemical methods is "questionable" for an additional 19%.

It's hard to even nit-pick this kind of information when the basic assumptions, data base, etc. are not provided. The point here is that the information is so vague and anecdotal that it hardly provides a basis for reasoned decisions.

EXCESSIVE REFORESTATION LAG TIME:

Other information comes through more clearly. Accomplishment records from the Annual Reforestation and TSI Needs Reports for the Siskiyou NF show that since 1975, each year has ended with at least 4,500 of the cut-over acres in need of planting left over to be planted another year. [See the graph in Attachment #4.] With an average yearly harvest during this period of over 4,000 acres, this means that all plantations have experienced at least a year of bare ground before they are planted. Current needs on the Siskiyou NF at the end of FY 1987 (before the wildfires) were 7,832 acres.

It's well known that prompt planting is the best way to reduce needs for herbicides in site prep, or for any kind of release, so what's going on? Is this situation typical of other forests in the Region?

In the Siskiyou NF's 1/9/87 report to the VM EIS team, they specified "immediate (same season) replanting" as an important tool to minimize site prep herbicide needs under Alternative D, but mentioned that "This technique is limited by availability of suitable seedlings and acceptable planters."

Given the importance of prompt planting, one would hope for Regional direction that this issue be given top priority. As it stands now, the Siskiyou NF seems content to let its large backlog of reforestation needs remain intact. The most recent Reforestation and TSI Needs Report projects an average of only 5,143 acres to be planted annually for the next 5 years. With projected harvests in excess of 4,000 acres, and an expected 25% rate of reforestation failures on first plantings, no headway will be made on the backlog, and the brush will still get a 1 to 2 year headstart on the planted trees.

Improper management of reforestation damages the credibility of the Forest Service to perform other vegetation management activities effectively and efficiently.

EXCESSIVE LAG TIME IN RELEASE TREATMENTS

As with reforestation, release needs on the Siskiyou NF have remained well in excess of annual accomplishments. [See the graph on p.2 of Attachment # 4.]

It is unfortunate that the Siskiyou NF entered the time of the Herbicide Ban with such a large number of acres (24,444) reported as in need of release, despite the record efforts of 1983. During that year, after Federal District Courts had ruled worst case analyses as necessary for herbicide use, the hand-writing was on the wall, and over 10,000 acres on the Siskiyou NF received aerial spray.

Ironically, while the helicopters were working overtime to get a jump on the courts, silvicultural exams found a record number of acres in need of release treatment. [13,227 acres. See the Reforestation and TSI Needs Report for FY 1983.] Whether these greatly increased needs were the results of past

herbicide failures is not reported ... but it's exactly the kind of stuff that we need to know.

WHAT'S BEEN DONE SINCE THE BAN?

The average number of acres given release treatments each year from 1984 through 1987 was only 1,475 [from the Siskiyou NF Annual Reforestation and TSI Needs Reports]. When this is compared to the yearly average of 6,093 acres given release treatments from 1975 through 1983, several questions are raised:

1. Are only 24% of SNF acres suited to nonchemical release? $[1475/6093 = 0.24]$
2. Are silviculturists learning to cope with the changed situation?
Non-chemical treatments have increased steadily from 648 acres in 1984 to 2,662 acres in 1987.

3. Do silviculturists expect a return to herbicide use in the near future?
The five year projection of the TSI program given in the 1987 report shows only 1,648 acres scheduled for release in 1988 ... but then something changes dramatically, and the annual release acres projected for the years 1989 through 1992 average 10,331 acres/year.

This last question underlines a problem at the root of these comments. The Siskiyou NF silviculturists are willing to project an accelerated program (apparently with herbicides) in order to reduce their backlog of release needs, but they are unwilling to embark on a similarly accelerated program to reduce the backlog in reforestation needs...despite the fact that a reforestation lag increases the need for release. What kind of biases are we dealing with here? Can Forest Service silviculturists trained in herbicide use, and often contemptuous of claims regarding toxicity, be trusted to implement the Integrated Pest Management that the Forest Service is mandated to follow whether or not Alternative D is selected as preferred?

For one indication of this bias, please refer to page 2 of the newspaper article included as Attachment #5. Here, Warren Olney, public affairs officer for the Siskiyou NF, is quoted as saying that "the Forest Service believes herbicides continue to be the safest and most effective method of controlling vegetation..." With this kind of mind-set prevalent in upper echelons of the Forest Service, how can people have confidence that herbicides which are to be used only "as a last resort" under Alternative D will not be used immediately, with no effort to find alternatives first? If Alternative D is implemented, especially with the present all-too-vague guidelines, it seems that citizens will have to watchdog every single herbicide EA, in order to avoid abuse.

IS FOREST SERVICE DATA RELIABLE?

The figures for release accomplishments given in the preceding section were taken from the Annual Reforestation and TSI Needs Reports. Unfortunately, they differ markedly from the acreage figures in the Siskiyou NF's "Final Attainment" report. For the years 1984-1986, either 648 or 291 acres were released in 1984, either 1,037 or 902 for 1985, and either 1,553 or 5,460 for 1986.

If contradictory information exists for such a simple matter as acres treated, what kind of problems will turn up when we begin to look at more complex measurements such as young conifer stocking and growth? Unless knowledge about existing plantations becomes a top priority for the Forest Service, a cloud of excessive controversy will continue to surround its activities.

WHAT IS NEEDED: FOREST-SPECIFIC EISs.

1. The VM DEIS applies everywhere in general, but nowhere in particular.

There is simply too much variety between all the vegetation types of Region 6 to be dealt with in a single EIS. The greater perceived need for herbicides on some Forests is masked by the averaging procedures of the analysis.

2. Excluding harvest practices from the analysis is a fatal flaw.

The assumption that alternative harvest practices will be adequately dealt with in the Forest Plans is incorrect. For the Siskiyou NF Plan, the discussion is cursory at best, is the same across all Plan Alternatives, and contains no discussion of implications with regards to vegetation management (site prep and release).

If it wasn't for the controversy about clearcutting, there probably wouldn't be a National Forest Management Act. Yet, consideration of this practice, and its relevance to successful reforestation, literally falls through the cracks in the Forest Service's planning process. No single document covers the entire sequence from tree-falling through site prep and conifer regeneration to release and thinning operations. Clearly, this must be done, and can only be done adequately at the level of each individual national forest.

3. Reforestation success, in the broadest sense, is the key issue. Therefore, look at what's been done already. Before planning for the future, evaluate the past.

As pointed out in Attachment #6, the issue of reforestation success is avoided by the forest planning documents. Adequate analysis of this issue must be based on a full inventory of existing plantations in terms of stocking and growth. Completion of such an inventory, along with continued monitoring as

specified in the Forest Plans, will provide an essential basis for the additional EISs proposed here.

It's crucial that this plantation inventory record changes in native plant species diversity due to management activities. These changes are important not only for the question of diversity per se, but also for the question of beneficial functions performed. For example, if manzanita provides a host for species of mycorrhizal fungi symbiotic with douglas-fir, then the presence or absence of manzanita could be essential to vigorous douglas-fir survival and growth.

The primary goal of the vegetation management of "competing and unwanted" vegetation is reforestation success in the broadest sense ... the maintenance of long-term site productivity and a sustained yield of timber. Only by looking at the full sequence of USFS-approved actions, on the individual Forests where they occur, can an adequate understanding of consequences, risks, and possibilities be achieved.

CONCLUSION

The fact that this current DEIS is premature and inadequate (due to incomplete information) should not be a cause for discouragement.

The present documents represent one significant step forward in what will be a long and monumental journey: namely, a complete analysis of the Forest Service's entire reforestation program. Eventually, and hopefully soon, this analysis will proceed from the ground up, from existing plantations to future stands of timber. Up until now, analysis has proceeded from the top outward, from statistical samples to theoretical yield tables and regressed growth curves: and then from hypothetical future yields and allowable cut effects to annual harvest levels now.

The current Vegetation Management DEIS, by its very incompleteness and the tantalizingly few research studies, clearly points the way for future efforts to follow.

For the sake of the Forest Service's continued reputation as a steward of the public lands, one could hope that the question of herbicide use will be put off indefinitely to the far future, so that people can go on with the creative work of managing without chemical poisons of dubious necessity.

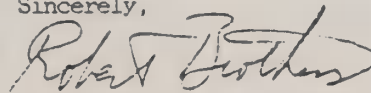
The methods of Integrated Pest Management (which the Forest Service is legally obliged to follow) provide an excellent model for future activities. The emphasis on preventing problems before they occur, on closely monitoring all actions, and on awareness of complex consequences is especially necessary when considering the management of human activities within natural ecosystems.

However, before any improvement can occur, some difficult facts must be faced. Up until now, the Forest Service's "can-do" attitude has caused it to gloss over problems which can no longer be avoided.

Under Alternative D, herbicides are specified for use as a "last resort" (see Attachment #7) for a good reason ... they're killers. In order to avoid causing unknown individuals injury, sickness, or death, some other methods must be employed. These methods may cost more money, and yield less theoretical timber, but these disadvantages do not make the unacceptable any more acceptable.

Thank you for reading these comments. I look forward to continuing interactions over these and other issues. The closer we look at what's going on in the forests, the better we will understand how to receive gratefully all the benefits that Nature provides, and that we scarcely deserve.

Sincerely,



Robert Brothers
for Earth First! Siskiyou

*P.S. There really are some good things
in this EIS, and the compliance
with NEPA and the CEQ regs
is (with some exceptions) the best
that I've seen.*

COMMENT LETTER: Vegetation Management (Herbicides) EIS for Oregon & Washington

Please mark the points that you agree with below,

Add comments of your own, and then MAIL TO: Gary Larsen, USFS Region 6
POB 3623, Portland, OR 97208

COMMENTS DUE JANUARY 15TH

*List of Specific
Comments*
↓

THE HERBICIDES ARE COMING!

The Forest Plan assumes the use of herbicides, so, lo and behold, right on its heels comes another document that seeks to legalize the use of these poisons again on our public wild lands.

The moderate groups are caving in on this one, and are backing an alternative which allows the Forest Service the use of herbicides as "a last resort". The rationale here is that the legal teeth which stopped herbicides 4 years ago can't hold much longer, so we'd better settle for an alternative which would cut herbicide use by large amounts, and which actually has a chance of being adopted.

Such a compromise may be tempting for those who have struggled long and hard for alternatives to pesticides, but the Earth can accept no compromise when it comes to long-term poisons, carcinogens, or genetic code scramblers — neither can Earth First! Siskiyou.

Do you remember the so-called "spray-wars" of years past, when citizens were sprayed with poisons while standing in clearcuts, when Forest Service officials lied and tried to sneak back in the next day to spray by hand, when folks from Wolf Creek occupied the Medford BLM office?

Let the Forest Service know your experiences with birth defects, miscarriages, and illnesses from herbicides, and how you feel about it. Someone must speak for all the plants and critters. Who knows about deer babies born without brains, about mountain lion miscarriages, or what kind of seeds will come from doug-fir trees exposed to mutagens? Last summer's Spotted Owl survey found only one healthy juvenile amongst many adults. Could that be because the Siskiyou National Forest has had by far the greatest annual herbicide use in all of Oregon and Washington?

Keep the Forests Toxic-Free Zones!

Included below is some letter-writing information purloined from moderate environmental groups. If it wasn't for some of these local folks, the Forest Service might still be spraying, and their comments are right on target.

HERE'S SOME KEY POINTS FOR YOUR COMMENT LETTER, PLEASE MARK THE ONES YOU ENDORSE

-all of them!

1. The DEIS presents many uncertainties about the silvicultural effectiveness and toxicity of herbicides. Describe the relevance of these uncertainties to their possible effects (worst case analysis: dead trees, dead people).
2. Describe the relevance of the data gaps concerning the 45% of commercial forest land with vegetation types where the needs for herbicides are unknown.
3. Analyze previously sprayed areas for long-term effects, especially on "non-target" plants and animals, endangered species, etc.
4. Admit to significant data gaps concerning the full formulation of chemicals that actually gets sprayed, including so-called "inert" ingredients whose identity is protected by trade-secret laws.
5. Describe and consider the beneficial effects of other plants in the tree plantations (nitrogen fixation, shade and shelter, slope stabilization, etc.)
6. Analyze the effects on local employment of manual release vs. aerial spray.
7. Add implementation guidelines to ensure that individuals exposed to toxic chemicals do so only with their "informed consent".
8. Consider the establishment of Toxic-Free Zones surrounding salmon spawning beds, important wildlife habitat, and all residences.
9. Put stronger teeth in the decision-making structure for Integrated Pest Management in Alternative D, to ensure that all other options are fully explored before herbicides are considered for use as a "last resort".
10. Analyze the historical records of each National Forest regarding the assumptions made about the efficacy and costs of different treatments.

*- Robert Burton
EFL Siskiyau*

000238

1064 W. 5th Avenue
Eugene, OR 97402

15 January 1988

Gary Larsen, USFS Region 6
P.O. Box 3623
Portland, OR 97208

Sir:

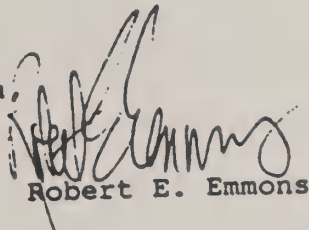
Considering the vegetation management EIS for Oregon and Washington, please accept the following suggestions:

1. The DEIS presents many uncertainties about silvicultural effectiveness and toxicity of herbicides. Describe the relevance of these uncertainties to their possible effects (worst case analysis: dead trees, dead people).
2. Describe the relevance of the data gaps concerning the 45% of commercial forest land with vegetation types where the needs for herbicides are unknown.
3. Analyze previously sprayed areas for long-term effects, especially on "non-target" plants and animals, endangered species, etc.
4. Admit to significant data gaps concerning the full formulation of chemicals that actually gets sprayed, including so-called "inert" ingredients whose identity is protected by trade-secret laws.
5. Describe and consider the beneficial effects of other plants in the tree plantations, re: nitrogen fixation, shade and shelter, slope stabilization, etc.).
6. Analyze the effects on local employment of manual release vs. aerial spray.
7. Add implementation guidelines to ensure that individuals exposed to toxic chemicals do so only with their "informed consent".
8. Consider the establishment of Toxic-Free Zones surrounding salmon spawning beds, important wildlife habitat, and all residences.
9. Put stronger teeth in the decision-making structure for Integrated Pest Management in Alternative D, to ensure that all other options are fully explored before herbicides are considered for use as a "last resort".
10. Analyze the historical records of each National Forest regarding the assumptions made about the efficacy and costs of different treatments.
11. PREPARE A FULL, FOREST-WIDE "REFORESTATION EIS" for the Siskiyou NF. This study should incorporate information learned from a full monitoring of all existing plantations of all of the various vegetation types unique to the Siskiyou and not described in any existing document. Within this context of known and well-evaluated past practices, it will be possible to look at the consequences of alternative methods of timber harvest, reforestation, release, and thinning.

Without this Reforestation EIS, there will be no way to apply the alternatives in the present Vegetation Management DEIS to the unique problems of the Siskiyou National Forest.

12. PLEASE ADOPT ALTERNATIVE C. The health of the forest,
the creatures within it and the people who enjoy it is of utmost
importance.

Thank you



Robert E. Emmons

cc: Rep. Peter DeFazio
Senator Bob Packwood
Senator Mark O. Hatfield

Hager 1

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CRITIQUE OF DEIS FOR VEGETATION MANAGEMENT

I am generally supportive of this effort as compared to previous efforts. The way of dealing with the problem of uncertainty is more realistic, there is an alternative that is recognizable as being influenced by IFM, and the information is for the most part well presented. Although the regional nature of the document makes it impossible to answer the question "how does this impact me?", this may actually be a strength, forcing us to take a broader view of the situation.

In the material that follows, I will first make some general criticisms, then point out the advantages of alternative D, then discuss the disadvantages of alternative D, and lastly list errors found in the DEIS. Because of the size of the Appendices volume, I have not reviewed it in any detail, so lack of criticism in no way indicates approval of what is contained in that volume.

The Social and Economic Analysis is inadequate as a decision making tool because the baseline to which it is referenced (the sum of the Forest Plans) is not available for inspection. The jobs calculation is a particularly good example of the problem with a floating reference point. In D, the projection is for -3100 jobs relative to those projected in the Forest Plans. If the Forest Plan projections sum up to +13100 jobs, then this apparent loss of jobs is in reality an employment boom! In the preferred alternative of the Siuslaw Plan, the projection was for a 10% increase in employment over the next ten years. Thus, no decision should be based on the Social and Economic Analysis. This is not a fatal flaw in the DEIS, however, because the modelled variations are "well within the usual year-to-year variations in employment, income and payments seen in the Pacific Northwest." (p. II-30).

The discussion of "brown and burn" in the DEIS is deficient. It says "This practice involves spraying of vegetation with herbicides in order to desiccate (sic) it. Five to seven months later, the site is broadcast burned." (p. IV-21). Compare that with Appendix D, Section 5, p. 11: "Typically the selected herbicide is applied aerially in the fall and the vegetation is not burned until the next spring,

approximately 5 to 7 months later. However, in some cases, burning may take place as soon as 2 weeks after the herbicide has been applied." (my emphasis). The calculations purporting to show that the risks to workers on such a fire are negligible are speculative at best. Brown and burn should not be used.

If the herbicides 2,4-D, Amitrole, Diuron, Fosamine, Bromacil, 2,4-DP, and Simazine are sufficiently dangerous to warrant their exclusion in a risk prevention alternative, they should not be used in any alternative.

The formulas for the risk indices should be given so that people can see how simplistic they are. I think they are:

$$RI(\text{public}) = PF + 3 * WF + AH \text{ (all in 1000s of acres)}$$

$$RI(\text{worker}) = PF + 3 * WF + TH$$

The DEIS characterizes Mechanical treatment as having low risk to human health (p. IV-124). Therefore, along with biological control (planting, grazing, etc.) it should be the method of choice on roads since compaction of soils is obviously not a problem on roads.

Alternative D is predicted (Figure II-3, p. II-22) to result in a decrease in the Long Term Sustained Yield Capacity (LTSYC). This appears to conflict with the text (p. IV-134) which says, "Alternative D will have a slight positive effect on long-term productivity....". Probable short-term reductions in timber yields (II-43) are acknowledged, but these should not affect long-term capacity.

To be told that "Most herbicides are relatively immobile in soils and do not persist long enough to travel more than a short distance through the soil (Norris et al., 1983)." (p. IV-45) is not helpful. The DEIS needs to clearly identify those herbicides with the greatest potential for leaching, and limit their use on watersheds where down-stream uses include human consumption or agricultural usage. Picloram, 2,4-D and Triclopyr are quite mobile, as is indicated in Table IV-13 (p. IV-47).

From the information presented, I am unable to decide whether the projected increase in wildfire is a serious

drawback to alternatives C and D. I have been unable to determine the source of the data on which the estimates of increased wildfire acreage due to decreased prescribed burning acreage are based. Tiedemann (1978) does not approach the problem quantitatively, but cites Gale (1977) as to the inevitability of wildfires due to accumulation of fuels. Gale (1977) is a USDA-FS Policy Analysis Staff Report, not the kind of peer-reviewed, publicly available evidence I would prefer.

If 27% of the wildfire acreage is due to escaped prescribed fire (1974-1984, p. IV-29), and the average annual wildfire acreage is about 21000 acres (1977-1986, p. IV-57), then about 5700 acres of wildfire per year are due to prescribed burning. Thus, a reduction in prescribed burning of 60% from B to D should lower the wildfire acreage by about 3400 acres. It is not indicated whether this factor was included in the calculated acreages given in Tables IV-11 (p. IV-29) or IV-12 (p. IV-37).

Additionally, Tables IV-11 and IV-12 are very misleading because the prescribed fire acreages given are low by about an order of magnitude (cf. Table II-10). Also, comparison of these two tables would lead to the conclusion that wildfire in the second decade is the same as that in the fifth decade, since the numbers are the same but the labels different. I suspect that Table IV-12 also should refer to decade five in its caption and body.

In Table IV-12, in decade "2" (=5?), the increase is only 4000 acres relative to the historical average. I suspect that the yearly fluctuations are substantially more than this, as is also suggested by the difference in the 1971-1986 average (Table IV-11) and the 1977-1986 average (Table IV-12). Tiedemann (1978) notes that "alternate means of slash disposal and fuel hazard abatement, coupled with increased efficiency in fighting fire have reduced wildfire acreage." (my emphasis). Because wildfire incidence depends on ignition source (and weather) as well as available fuel, it is certainly possible that increased prevention and suppression efforts could more than offset the fuels buildup problem. Thus, until the relation between reduced use of prescribed fire and increased acreage of wildfire is better documented, projected wildfire acreages should not be used in the decision making process.

Alternative D (hereafter referred to as D) is clearly the best of the preferred alternatives. In some ways C is superior to D, but I consider C to be more of a benchmark than a real alternative. Thus, C is excluded from this discussion to eliminate having to say "except for C" in many statements.

Advantages of D include:

1. Lower risks to human health (as defined).

- Lowest acreage of prescribed fire (p. II-24).
- Lowest acreage of manual treatment (p. II-24).
- Lowest acreage of aerial herbicide, except those alternatives in which it is banned (p. II-24).
- Lowest acreage of total herbicide, except those alternatives in which it is banned (p. II-24).
- Lowest worker accidents (p. II-25).
- Lowest smoke (sus. part. (2.5 um dia., p. II-37).

2. Lower impact on the environment.

- Lowest mechanical treatment; " low risk of impacts on the soil and long term productivity" (p. II-38)
- Low wildlife effects (p. II-40)
- Lowest spreading of toxins and unknowns.
- Lowest use of fire.
- Lowest total acreage treated.

3. Effective.

- Ranks only behind B and G in effectiveness (p. II-41).
- Costs of vegetation management decline through time; therefore increasingly economically efficient (p. B-29).
- Road maintenance costs two decades and beyond are lowest of all alternatives (p. IV-90).
- Tendency to become more effective and efficient (p. II-43).
- Tendency to encourage more creativity in dealing with site-specific problems (p. II-43).

Problems with D include:

1. Too much herbicide use.

This is potentially a fatal flaw in D. Our ignorance of herbicide use extends even to so well defined a

question as "What chemicals are being sprayed?". Significant uncertainties with respect to human health effects are admitted. Thus, it's shocking that the alternative stressing prevention and substantially lowering total acreage only produces a 55% decrease in herbicide use relative to the forest plans. As a percent of total acres treated; in B, herbicides are 10.8%, and in D, herbicides are 7.0%. This is much too high for a "last option" treatment. I will be unable to support this alternative unless the herbicide acreage is substantially reduced.

2. Low budget may imperil adequate implementation.

Because some of the techniques that will be utilized in D are in the being-learned stage, the budget should be supplemented to allow for the necessary research.

3. Last option needs better definition.

Herbicide use as a last option is one of the key provisions of IPM. To demonstrate last option, it is necessary to show that other alternatives are incapable of achieving the goal of the action. Project costs are not an acceptable criterion for rejecting another method of treatment in favor of herbicide use.

4. Prevention not clearly distinguished from early treatment.

Prevention based on an integrated view of the system is a key to the desirability and efficacy of D. Prevention involves cultural practices which begin with harvest and which involve climate, microclimate, slope aspect, and existing vegetation and wildlife.

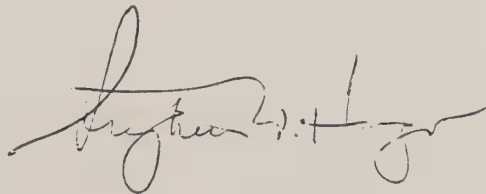
5. Wildfire hazard may be a problem.

I am unable to evaluate this possibility because of the lack of reference to substantiating information (see general discussion of the use of Fire).

6. Reductions in jobs, personal income, and payments to local governments may be significant.

I am unable to evaluate the impact on these factors using the information given (also see general discussion).

	ERRORS
page	error
II-86	Table II-10; "Receiving no treatment" row is not correct.
IV-125	Table IV-29; "Total:" row is completely wrong.
IV-25	Table IV-9 is out of place in the Soil Resources section. Not referred to until p. IV-46.
IV-47	Triclopyr reference missing. Probably 4.
IV-46	should read "0.1 mg/L (approx. 100 ppb)".
IV-47	There are 4 references which could be designated USDA-Forest Service, 1984. They should be distinguished as 1984a, 1984b etc.
II-22	Figure II-3; under E, the LSYC should probably read -1 to -1 1/2 %.
II-73	"steep (less than 60 percent) slopes" should read "greater than".
II-24	Table II-1; should read "Thousands of Acres Treated", not "Acres Treated".



STEPHEN W. HAGER
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Headwaters

For the protection of critical watershed

001312

February 15, 1988

Mr. Gary Larsen
USFS-Region 6
Portland, Oregon

Dear Mr. Larsen,

Thank you so much for the opportunity to participate in the preparation of the Vegetation Management Environmental Impact Statement for Region 6. This Draft is certainly a good start, and we offer the following comments for your use in the preparation of the next version of this EIS.

I know your team has worked long and hard to document this very serious issue. Having the court reject the first EIS has made this task more challenging. I am so thankful that you have been receptive and enthusiastic to the input from the Mary O'Brien, Norma Grier and others from the pesticide and forestry reform movement. Your interaction with the public has been noteworthy.

In this climate of increased communication, Headwaters feels sure that we can work together produce a legal and sensible analysis and risk assessment that will result in a positive change in the Region 6 forestry program. Please accept the following suggestions and critiques with that mutual goal in mind, as part of the necessary (though sometimes arduous) feedback/discussion process that is our mechanism for deriving a practical and safe solution to the vegetation management problem.

We look forward to discussing these issues once you have had a chance to absorb this round of input. Please notify us in April or May so that representatives from Headwaters may meet with you.

Sincerely,

Julie Kay Norman
Coordinating Council

HEADWATERS COMMENTS ON VEG. MGMT. EIS

USFS R6 - FEB 88

CHAPTER BY CHAPTER COMMENTS, QUESTIONS AND SUGGESTIONS

COMMENTS ON DEIS AS A WHOLE

1. Thank you for all the pretty maps and pictures with captions in this book! That is such a nice improvement over the Siskiyou Forest Plan, which does not contain 1 single picture. How did they expect to depict the forest environment without pictures? [The fact that there were no pictures made me suspicious of the whole document. Who's in control of this forest management bureaucracy anyway?]

These "suspicions" will not be dealt with further here, but I think that the cause for my suspicions is real and deserves immediate attention in SOME kind of EIS that addresses the environmental impacts of politics. Right?

2. Thank you for a pleasing document to look at with the very nice graphs, maps, tables and graphic representations of alternatives, decision flows, and especially the threshold curves. That's a first!

3. Notes on layout and format: I liked the colored paper and the big print.

4. Sometimes I got a little lost in the chapters. How about putting tables of contents behind each colored page to show what's coming up.

5. Thanks for mentioning the Law.

SUMMARY

1. Page 16: Restrictions on herbicide use:

a. Regarding Item #1, USFS and the public cannot rely on EPA label instructions. Given the continual litigation over toxic harm, it would be

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foolish to proceed with the level of information on the label or in the EPA files. The EPA has been fooled before, but the incidents of toxic poisonings blossom forth in the late 1980's and citizens begin to sue for damages and deaths (see attachments on dead child in Tuscaloosa and dead Rogue River National Forest worker). What potential is there for the Forest Service to be sued along with the offending chemical company in suits such as Greenhill v. Dow, the December 1987 case in Harrison County, Texas? Is the Forest Service ready to make a decision that would place them vulnerable to toxic torts?

According to lengthy investigation into chemical company behavior patterns, we cannot trust the companies to turn in the true data describing the hazards their chemicals produce in the manufacturing and application processes. These companies produce hazardous waste and they distribute it. Their lack of accountability and responsibility has been well noted worldwide; the recently discovered Krumel Report by a Dow chemist in Midland, Michigan revealed incriminating information about 2,4-D that the EPA had never seen. (See attachments on Chemical Company Track Records)

I don't know how to rewrite this "safety rule", because I don't think we know enough about toxic chemicals to say hardly anything about it. For example, we at Headwaters are very concerned about dioxins and inerts which are virtually undescribed in the DEIS yet remain a threat to vegetative and human life.

b. Please add a fourth restriction on herbicide use: #4 insuring a policy of "informed consent" for citizens and workers.

The concept of "informed consent" is crucial to the use of toxic chemicals. See comments by Steve Van Strum.

2. Page 17: Unavoidable Adverse Effects:

a. The DEIS states: "The [environmental] effects of most of [the alternatives] vary little from one alternative to another." This is false; it does not portray :

- a. the complexity of the problems and solutions you analyzed
- b. the interrelated nature of the resources and activities you are studying
- c. the potential for severe (direct, indirect, cumulative) adverse effects over time and space, most notably the toxic effects of herbicides on humans

Please change this sentence to portray the depth and difficulty of the analysis and describe the variations in risks across the alternatives.

b. List of 3 Areas of concern with significant adverse impacts: Yes, you've got these on target and in a good order.

The "economic effects from changes in timber yields" you refer to here need further documentation and analysis in order to weigh them in decision-making. The analysis contained in this Draft needs more work on firming up the economic assumptions in calculating "economic effects" triggered by "changes in timber yields", as explained in Appendix A, Appendix B and below.

Changes in Timber Yields: The inventories used for this Draft need updating and expansion to recompute field data summaries describing changes in timber yields

* PAGE NUMBERS ARE OFF BY 3... WOOPS.

as a result of changes to vegetation management techniques. Efficacy data needs recomputing based on recent experience with alternative methods and comparative studies for veg. mgmt. methods. Combine this with latest versions of timber yield tables to recompute growth rates for each type of vegetation management, incorporating plantation survey data for all plantations and natural regen stands as well. All progressive, low toxicity methods should be analyzed.

[Monitoring data is our most important tool. We've got the computers to crunch it. It's time to start doing it BIG TIME!]

Economic Effects Triggered by Changes in Timber Yields: I propose that you relook at the economic assumptions in your simulation model, so that it represents more than just the "best case" of costs and accomplishments, revenues and demand. New information on fossil-fuel price increases, slowed GNP-growth, increased expense for personal injury lawsuits and increased mitigation costs should be added to the cost/benefit model. The model should also reflect future revenue losses due to environmental degradation from repeated, intensive industro-forestry projects on the private and public lands as a whole. In terms of jobs and personal income, IMPLAN should be replaced with a model that really analyzes the demography and looks ahead to future trends such as decreasing jobs due to resource depletion and increasing transfer-payment revenues from retirees. Redefine model inputs to reflect this more realistic future, and then assess the alternatives based on these revised assumptions.

CHAPTER I: PURPOSE AND NEED

1. Page 5: Scope:

The DEIS does not illustrate a clear need for the intensive vegetation management program (proposed in all Alternatives but C) to "release controlling vegetation that competes with young conifer seedlings." Analysis of operational monitoring data and comparative studies are lacking to prove there is a need for such a large program. Only 55% of the forest base is included in the analysis of the 6 primary forest types, so 45% remains undescribed and unknown in terms of brush control needs.

Please gather the necessary growth and mortality data and compile comparative studies to quantify the needs. Expand the inventory base of distinct forest types to include at least 90% of the forest base.

CHAPTER II: THE ALTERNATIVES

1. Page 32: next to last paragraph: Recreation and forest workers find poisons more than "distasteful". Hunters, fishermen, berry pickers, timber stand improvement staff, surveyors, and children are all put at greater risk if herbicides are being used. Consequently, they perceive risk and experience real fears.

2. Page 32: last paragraph: The analysis of the relationship between changes

HW Comments 6

in vegetation management and level¹ of timber offerings is not adequately documented in the Draft, because monitoring data on efficacy and reforestation growth rates are inadequate at this time. Please rework this analysis using updated and expanded operational data. Please describe the degree of "speculation" on the predicted volume losses for individual Forests and across the Region.

3. Page 33: Yes, you have a big job because it is VERY complicated.

4. Page 40: Effects on Wildlife:

I disagree that Alternative C creates the highest long-term risk. The studies you cite are inadequate to draw this conclusion. What data do you have which says wildlife need human intervention?

Yes, your "lack of study" indicates a need for further research. Conclusory remarks scattered throughout the DEIS should be modified to emphasize the lack of data at this time.

5. Page 51: Implementing the Alternatives:

Congratulations on getting down to the nitty gritty on implementation here, with the beginnings of a procedure based on classical IPM management. I urge you to continue to work with Mary O'Brien and Norma Grier in developing this section so that it is a system that citizens can have confidence in. We just want to feel safe from harm, and a rigorous, structured decision-making process is the means to that end.

6. Page 55: Thresholds:

Your concept of defining damage and action thresholds is good. Monitoring data is the key to setting them. Please provide for a good inventory specialist and computer data analyst to each Forest for this very important job.

7. Page 57: Four Strategies:

Prevention is defined poorly here, as if it were merely early correction. Please use the classical IPM definition of "prevention". This would include not using silvicultural methods which aggravate the problems, such as even-aged, clearcutting. There are compromises to be made between the economic advantages of clearcutting and the silvicultural practices which enhance forest growth.

8. Page 57: Four Strategies:

The "prevention" strategy must be expanded to discussion how selection of harvest practices effects the degree of vegetation management needed. Add new strategies which capitalize on progressive combinations of harvesting and vegetation management, which result in "prevention", to one or more of the Alternatives.

9. Page 58: Human Health Assurance in the Design Process:

In the example using Alt. A, please express the difference in the degree of risk. Otherwise, it implies that there is a win-lose relationship between the public and the workers.

10. Page 60: Monitoring:

This is the most important part of the DEIS. Now, how can we make sure that monitoring gets funded????

- a. What is your system for funding these monitoring programs?
- b. Please define in detail the variables, data bases and yearly summaries/analyses that will be performed on each Forest to monitor vegetation management activities, impacts, and efficacy.
- c. Implement a full reforestation inventory on each Forest immediately to establish the baseline data for future comparisons.

11. Page 65: Human Health Risk Management Plan:

Good to see it. Please spell out the details of this Plan. Under what conditions does each planning element get triggered. List the variables and data bases for each Forest to monitor. If you choose to use herbicides, recognize that your herbicide application data could be very crucial to defending Forest Service practices in court.

12. Page 66: Forest Pest Management:

Please add: (c) information exchange takes place such that new toxicity information will trigger changes in policy of herbicide use.

13. Page 66: Reliance on Training for Safety:

Forest herbicide applicators have human frailties, such as laziness, just like the rest of us. Timber Stand Improvement teams have the potential to misuse toxic chemicals, in spite of the best possible training. Does the Forest Service feel it can rely on training and procedures to insure safe use given the level of risks?

14. Page 71: Prescribed burning - Mitigation of:

Given the adverse effects listed, soil damage, water quality damage, air quality degradation, escaped fire, and lack of predictability, the use of prescribed burning should be used sparingly.

a. The mitigation measures as defined are inadequate to prevent unacceptable damage, because the damage is so longlasting and difficult to quantify or predict. This section needs to be strengthened to truly address the long-term negative effects of burning.

b. What is the data on escaped-fire damage, and what are the historical and predicted losses of volume? Which part of the acres burned due to humans (68%...see page III-9) is associated with broadcast burning?

15. Page 76: Manual Methods:

Please quantify the advantages of labor-intensive, local employment for manual release methods as compared to capital-intensive chemical methods which employ

fewer local people and spend money⁴ on chemicals and helicopter fuel rather than payroll. Manual release contracts cost less to administer because there is less risk to be managed. Do your employment and cost estimates take all this into account?

16. Page 78: Herbicide methods:

a. First paragraph: As part of your assurances about EPA standards, describe the track record and flaws in the EPA registration and review process for herbicides. (see attached text by Samuel Epstein, Mary O'Brien, Carol Van Strum)

b. Last paragraph: Describe the growing trend of lawsuits being filed by citizens due to toxic poisoning.

c. New paragraph: Also describe the history of civil disobedience regarding herbicides.

17. Page 81: Herbicide Mitigation Measures:

a. Mitigation measure #1 is impossible to perform. History has shown that many people are unexpectedly harmed by exposure to herbicides and suffered acute and chronic illness.

b. Mitigation measure #2 does not protect the public given the current state of EPA registration and review, which is greatly hampered by secret reports and false industry analysis. The information on the label does not insure protection of the public or the workers.

c. Mitigation measure #3 is inadequate to insure protection because of climatic unknowns, unpredictable environmental conditions, and lack of data on persistence.

d. Mitigation Measure #4 needs to define these measures specifically.

e. Mitigation Measure #5 is of special concern due to increasing toxic waste streams all over. Where does the USFS recommend mixing and cleaning operations be performed such that they will not contaminate water? This measure should strike the words "whenever practicable".

f. Buffer strips of 100' and 200' have been shown to be inadequate to protect no-spray zones in recent studies by the State of Oregon and BLM-Coos Bay. Current understanding and lack of control of drift makes narrow buffer strips such as these inadequate to protect resources and health.

g. Please strengthen requirements and widen buffers in measures 6 and 7.

10. In mitigation #9, class IV streams should be protected with buffers.

11. Mitigation measure #10 should provide protection for backpack sprayers who ride in closed vehicles with poisons in the back.

12. Add a mitigation measure which addresses the issue of human fallibility.

CHAPTER III - AFFECTED ENVIRONMENT¹

1. This was a very interesting chapter, because it was the most real and contained some good information.

2. Page 11: Fire - future trends:

Is burning necessary to "preserve wilderness ecosystems"?

3. Page 21: Water:

Spell out why certain cities prohibited or severely restricted certain herbicides.

4. Page 23: Vegetation:

It's so satisfying to see an accurate account of how species diversity contributes to ecosystem health.

Please add discussion of the benefits from other species in terms of nitrogen fixation, shade, shelter, and slope stabilization.

5. Page 27: Forest Trends:

Last paragraph: Please delete it (bogus, misleading justification for harvesting). What "other management objectives" did you have in mind?

6. Page 32: Diversity inventories:

Do you have plans to create a diversity index and inventory as NFMA describes?

7. Page 38: Fisheries interactions:

This section needs expansion and more concise data descriptions. Please consider the comments written by Chris Frissell to the Siskiyou National Forest Draft Plan, which indicate radical degradation of water quality and fish habitat due to clearcut/slash/burn practices in fragile, steep conditions such as the Siskiyou and Siuslaw forests. This impact needs to be addressed more completely so that the true risks can be analyzed.

Are there any studies relating herbicide exposure to fish mortality?

8. Page 40: Recreation:

a. Is vegetation management really needed to "create and maintain a natural-looking environment"?

b. Add an "Interactions" section to describe the impacts to the safety and mental confidence of hunters, hikers, berry-pickers, forest workers etc. who want to eat and drink in herbicide spray areas. Describe how air quality degradation affects recreationists.

CHAPTER IV - ENVIRONMENTAL CONSEQUENCES

HW Comments 10

1. Page 7: Incomplete or Unavailable Information:

This section gets off to a good start, but the DEIS incorrectly claims that you have the information essential for "reasoned choice" in terms of (a) economic and (b) environmental impacts.

a. Economic Impacts: At this time, "an alternative's impacts...can be reasonably estimated", because the IMPLAN model relies on false assumptions. Since there is no data analysis to substantiate the gains or losses in timber volume with and without vegetation management, you cannot say that "there is sufficient information to provide a clear basis for making a choice among options with confidence." Due to the heavy emphasis in the model on timber volume and revenues, which rely on trended (1% annually) market values and statistically-derived yield tables that have not been ground-truthed, your claims about being able to make a clear choice are misleading.

Please implement the more rigorous standards of CFR 1502.22 in this discussion. Update the economic model and complete the analysis of existing data on vegetation management efficacy and related changes in tree growth so that realistic timber revenues can be calculated. Describe what you don't know and its relevance. Then evaluate.

b. Environmental Impacts: The DEIS claims that "environmental effects are reasonably well understood." Monitoring data for long-term clearcutting and vegetation management is not available to substantiate this statement.

Please implement the more rigorous standards of CFR 1502.22 in this discussion. Complete the analysis of existing data on environmental impacts, outline the information that remains undocumented, and describe its relevance. Then evaluate.

2. Page 10: Incomplete or Unavailable Information on Human Health Impacts:

The "Statement of Relevance" does not adequately describe the "relevance of the unavailable information"; it only restates that uncertainty exists.

Please rework and expand this section to describe the context and importance of the uncertainties in terms of analyzing impacts and risks. Tell how the unknown hazard factors effect USFS decision-making on herbicides.

For all significant variables in the risk analysis, make a table that shows:

- a. what is known and displayed
- b. what is known and not displayed
- c. what is unknown

This visual aid will help keep track of study data for the risk assessment phase.

3. Page 11: Evaluation of Impacts of Herbicides:

a. Since the Statement of Relevance is inadequate, your evaluation that "enough information is available that risk can be reasonably characterized for all herbicides except two, diuron and fosamine" is premature. Evaluation cannot proceed until the relevance (importance) of the unknowns is assessed.

b. How do you support the statement that "the risks to humans...is more closely

correlated to the number of acres treated, rather than to what tool is used"? Does this take into account the degree of harm associated with different methods? For example, is a sprained ankle from manual release work equated with the potential cancers, miscarriages, or neurological breakdowns resulting from herbicide use?

4. Page 13: Climate:

These two statements are contradictory, unless you have calculated the local changes to exactly balance out: "Veg. mgmt. activities are not expected to have significant or cumulative effects on the Regional climate." and "Local, site-specific microclimate changes are expected."

Your discussion of how the microclimate changes with clearcut harvesting and burning is good, and should be part of Forest Plans as you indicated. What are good sources for this information?

5. Pages 13 to 17: Soils:

Your data indicate many problems for long-term site productivity from vegetation management. Given the lack of key indicator variables and the difficulty in quantifying impacts over time, how can USFS be assured that they are not causing unacceptable degradation?

6. page 21: Herbicide Effects:

a. In regard to brown and burn techniques, please analyze the potential for the production of additional toxic substances/waste (such as TCDD) from incineration of herbicide formulations, and disclose the uncertainties for health hazards.

b. Where is the data supporting the fact that "90% or more of the herbicide used will be intercepted by foliage"?

c. Please discuss the potential long-term effects on non-target species.

7. Page 22 : Cumulative Effects:

The adverse cumulative impacts to the soils from clearcutting and vegetation management, such as mass-wasting, loss of soil productivity, and disturbed nutrient cycling and nitrogen-fixation, have not been sufficiently documented or analyzed to insure that unacceptable degradation is not occurring. Please expand the cumulative effects analysis to encompass these factors and disclose the uncertainties and relevance of the uncertainties. This in-depth coverage should appear in this EIS, so that duplication of effort will not be forced on each project-level analysis. Given the information in this draft EIS, a "reasoned choice" cannot be made, and protection is not assured.

8. Page 23: Herbicides: Synergistic Effects:

Data on synergism for full formulations of herbicides is inadequate. Please expand the analysis and describe the uncertainties and relevance.

9. Page 42: Water Quality - effects from prescribed burning:

Please present the data to substantiate the statement that "prescribed burning

generally does not affect water yield, erosion, or water quality during storm runoff."

10. Page 44: Water Quality - effects from herbicides:

How high could concentrations of herbicides be from "direct application or drift", and what is the risk associated with these short-duration exposures? Your drift data for 100 and 200 foot buffers is contradicted by recent studies by State of Oregon and BLM-Coos Bay. Drift control strategies are merely that, and do not insure protection.

11. Page 46: Water Quality - spills:

What were the impacts recorded from the 17 documented herbicide spills?

12. Pages 51 - Timber Yields:

As pointed out before (see Chapter I, Comment #2), timber yield predictions by alternative are unsubstantiated by post-treatment evaluation reports for the different methods of vegetation management. This is especially unfortunate because the calculated timber volume correlates directly with PNV, revenue, and employment projections, which are the key indicators for economic impacts.

a. Please rework the display of timber yields and related indicators to clearly show the level of confidence in these variables and admit to the current uncertainties.

b. The projected losses in yields do not match with previous projections of gains from herbicide use within the Forest Planning process. How can the allowable cut effect attributed to herbicide use, in the Siskiyou NF yield tables for example, be reconciled with the vegetation management EIS projections?

13. Page 63 - 70: Wildlife and Wildlife habitat:

Except for manual release, the direct and indirect effects of vegetation management to wildlife and wildlife habitat are severe, and the mitigation measures need to be strengthened to encourage the use of manual release. Studies on the impacts from chemical methods are inadequate to "indicate relatively low toxicity to wildlife" (p. 69). Please revise this claim.

14. Page 71 - 74: Riparian Resources and Fisheries:

Vegetation management impacts to fisheries may be minor compared to harvest and roading, but the combined cumulative, direct and indirect effects from increased sedimentation are significant for reasons cited in the American Fisheries Society comment to the Siskiyou Forest Plan and should be summarized in this EIS.

a. Please expand the analysis to encompass the concepts and studies cited in this comment letter and reassess the impacts for each vegetation management method.

b. Mitigation measures should be strengthened, because drift and accidental exposures pose definite threats to the public and environment.

c. Synergistic effects are not adequately estimated, because full formulations of herbicides vary by batch and include unknown ingredients. Please clarify uncertainties and their relevance.

d. On page 75, second paragraph, please revise the claim that "Mitigation measures...will prevent exposure to toxic concentrations of herbicides." Accidents, failed mitigation measures and unpredictable events make this statement untenable.

15. Page 77-78 : Recreation:

a. What are examples of other "toxic plants"?

b. There will be more than a "loss of opportunity" to pick berries: there will be a loss of opportunity to pick unpoisoned berries. Berry pickers have been known to pick sprayed berries along roadsides. How do you suggest warning all the berry pickers to minimize toxic poisoning?

16. Page 81: Rights of Way:

If you are going to mention the potential for tort claims because of lack of safe driving visibility, please discuss the potential for toxic tort claims from citizens and workers who are poisoned by herbicides.

17. Page 83: Energy Consumption:

a. Please spell out the petroleum needs for general management as compared to each of the vegetation management methods in dollars per acre.

b. Consider the increasing cost and scarcity of fossil fuel products over the next 100 years and add this variable to the economic model.

c. Consider the side-effect impacts on the environment and the economy from adding undesirable waste products (2,4-D sludge, dioxins, etc.) resulting from the manufacture of herbicides which adds them to the national "waste stream". Make sure the recycle and cleanup costs are reflected in the economic analysis, so the true costs of using toxic chemicals are reflected.

18. Page 91: Human Exposure to Herbicides:

a. Recent studies in Canada, Sweden, and the U.S. indicate that people who are repeatedly exposed have greater risks of suffering adverse health effects. If, as you say, "spray applicators are repeatedly exposed", the use of herbicide "hack and squirt" or "backpack" spray methods should be eliminated due to unacceptable risk. The USFS has a moral right to protect its workers in this way, because mitigation will not reduce the risk to acceptable levels.

b. Public exposures may "not be expected to occur more than once to any individual", but even this risk is unacceptable given the evergrowing data base on the severity of the acute and chronic effects to human reproductive, neurologic, and immune systems.

19. Page 94-100: Summary of Epidemiology for Herbicides:

HW Comments 14

Thank you for acknowledging the known studies and the many unknowns, including the studies on TCDD.

More human test data (worker files and clinical reports) are needed to beef up this section.

Also, consider the fact that poisons in forest use are contributing to an atmosphere and physical environment that is already polluted in other ways, so the toxic chemicals are not operating in a vacuum.

20. Page 107 : Table IV-25:

The scarcity of High (H) Confidence Levels indicates that routine risks to backpack sprayers are unacceptable, and that this method of applying herbicides should be eliminated.

21. Page 112: 2,4-D:

Please strike 2,4-D from the list of potential herbicides, based on the myriad findings that it is toxic to humans (acute and chronic), may contain dioxin, and results in toxic by-products during manufacture. See attached and incorporate the case against 2,4-D by Mary O'Brien of NCAP.

22. Page 116: Diuron and Fosamine:

Please strike Diuron and Fosamine from the list of potential herbicides, based on the lack of information.

23. Page 117: Inerts, Interactions, Impurities:

Since there "is no regulation of inert ingredients", "little or no information" on interactions, and no data on technical grade herbicides, the analysis of herbicide toxicity is incapable of assessing the true risk of adverse health effects. The relevance of these unknowns needs further description.

24. Page 119: Public Involvement in Risk

Rework this section to discuss exposures to timber stand improvement workers, surveyors, hunters, and other recreationists. Does your argument that "the public will seldom be exposed to herbicides" counterbalance the fact that there may be severe health effects to a small group of the public?

25. Page 123: Toxic Effects of Fire Smoke:

Please collect more data on the effects of air pollution from slash burning, and discuss the relevance of the uncertainties remaining.

26. Page 130: Effects on Communities

Revise the tables describing jobs and personal income across alternatives after modifying the timber volume changes as discussed above. Replace the IMPLAN model with a more complete, up-to-date economic model.

27. Page 132: Other Social Effects:

I/B Public Participation and Consultation

HW Comments 15

Given the level of public dissent and distrust of the Forest Service which developed in the pre-ban era, USFS should not expect "public contact and participation" to allay the public's fears of chemical exposure and accidents. This is an issue of personal values: poisons are unacceptable to more and more people due to the increasingly toxic nature of life in the 1980's. Protesters, letter-writers, and citizen organizers believe that "no amount of poison is safe enough."

Revise this section to realistically describe the reactions of these concerned citizens to the return of herbicides, including civil disobedience, media campaigns, and other forms of resistance.

See attached media clip from peaceful demonstration in Grants Pass Oregon on February 2, 1988.

SUGGESTIONS ON THE STRUCTURE OF ALTERNATIVES

1. From all alternatives, remove the use of herbicides until the data are available for an adequate health risk assessment.
2. For all alternatives, establish classical IPM procedures to analyze need and prescribe treatments for all vegetation management projects.
3. For all alternatives, establish monitoring procedures to produce necessary inventories for risk assessments of the vegetation management impacts to human health, forest health, and economic health.
4. For all alternatives, encourage exploration and rigorous study of alternative combinations of silvicultural and vegetation management practices (tailored to Forest-specific site conditions) so that vegetation management needs are minimized.
5. For all alternatives, minimize air pollution and degradation to soil/water quality by minimizing prescribed burning.
6. For all alternatives, incorporate the spirit of Alternative D to capitalize on natural processes and prevent problems.
7. For all alternatives, incorporate the spirit of Alternative E to emphasize protection for workers and prohibit burning of sprayed units (should they be allowed).
8. In summary: maximize safety
minimize need
and monitor results for frequent analysis.

000407

I.B. (Ben) Iverson

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Iverson

E.I.S. Response

October 1987

Dr. Jerry Larsen
Vegetation Management Group Leader
USDA Forest Service, Pacific NW Region
P.O. Box 3623,
Portland, Oregon 97208

Dear Doctor Larsen:

This is my report for your requested Public Participation to
"Managing Competing and Unwanted Vegetation", for Forest
Management.

I challenge your report as being a fabrication of wizardry and
a perpetuation of perfidy, and deception which has been foisted
upon us:

- (1) Herbicides do not disappear from the environment.
- (2) Herbicides are not selective as to the species which ingests
them.
- (3) The oil or resin content of plants or animals determines the
toxicity of a given herbicide, when ingested.
- (4) When ingested by plants the herbicide is only demobilized
temporarily. It will be found in the body of all plants equally.
Corn, Cotton, and softwood trees, (fir, pine, --) are better
equipped to demobilize herbicides in fats or resins.
- (5) When remobilized through burning, the herbicides will often
reappear as dioxins.
- (6) Herbicides are mostly insoluble in water. The final resting
place for them is in the bottom silt of bodies of water.
- (7) Herbicides are transported in water much the same as silt or
may actually be adhered to silt or clay particles.

These explain only a few of the misconceptions which have been
foisted upon us. The facts were well known by chemists more
than twenty years ago. For background, please see the McGraw-Hill
Encyclopedia, (1960) under the subject "Halogen".

I was interested in my local ecology in 1968. In 1972,
herbicides were introduced locally (Section 17, Twp. 24 N., Range 7
E.W.M.) by county road crews on roadside shoulders. Thus, began
my on-site investigation of its effects, when the sudden
disappearance of fish from the streams came to my attention.
Daily, weekly, monthly, and/or yearly observations were
recorded. That is the documentation upon which the following is
based.

The most preliminary consideration is missing from the
Environmental Planning Study. That consideration is that the
environmental Protection Agency (EPA) is currently doing a restudy

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of all Pesticides. As a result, Practically all of the references cited in Your study are obsolete. Until the EPA approves new or old herbicides, their outcome is in doubt.

Most herbicides are in doubt because of some documentation I have accumulated over the Past fifteen Years. Before getting into the documentation, an outline of herbicide formulation, Ground Pathways, longevity, and biological consequences, is in order:

FORMULATION

Practically all herbicides are halogen based or based on the Pseudo halogen, cyanide. The halogens are Fluorine, Chlorine, Bromine, and Iodine. One or more of these halogens are attached to a hydrocarbon molecule in a simplified description. Because they are attached to a hydrocarbon molecule they are ingested by Plants and animals for the hydrocarbon content.

PATHWAYS

A curious feature of these halogenated hydrocarbon molecules is that they are almost universally soluble in olefins, or oils, fats, and greases, but relatively insoluble in aqueous solutions. There is little actual difference between them and the commercial degreasers. The body of the organism ingests this halogenated hydrocarbon molecule for metabolic needs, but finding the halogen attached, the body, usually the liver, finds it cannot utilize it without risking Poisoning. Trying to excrete this molecule, the body finds that it cannot dissolve it in ordinary bodily fluids. It can only dissolve it in the olefins in the body, and these fats cannot be excreted through normal channels. It can be excreted only through the oil glands and through mother's milk. (Hence liver damaged infants). One of the early symptoms of herbicide Poisoning is acute acne. These fats must be stored within the system, and there is therefore a weight gain connected with herbicide Poisoning. Before the weight gain there is a demand by the body for more food with which to create the fats which will absorb the halogenated molecules. This is probably the most reliable symptom of herbicide Poisoning in the earliest stage. That is, there is a greatly increased hunger to the extent of being famished. It occurs even in minor Poisoning incidents, but passes unnoticed, although it is noted in nearly all test results. "The test specimens gained weight". Farmers also find it useful in fattening livestock. This is not an innocuous chemical.

In Plants, the halogenated hydrocarbon is ingested through the roots and through the leaves. Very little of it remains in the older growth. It is preferentially delivered to the newer growing parts, (This will cause diploid growth [double tops] in timber

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species), and to the flowers and seed. It occurs particularly in the seed if those seeds contain oils as do corn and cotton. Hence they are quite tolerant of halogenated hydrocarbons. On the other hand plants like melons and lettuce contain little oil and are extremely susceptible. It is little wonder that the poisoned watermelons were raised in California, without any addition of herbicides prior to or during the growing season.

LONGEVITY & TRANSPORT

This last statement also gives a lie to the statement of longevity in your report. Anyone manufacturing or using these halogenated herbicides must be a magician, an alchemist, or a wizard in the claim that the herbicide disappears in a matter of hours, days or even weeks or months. Chlorine can be neither created nor destroyed. Surely a small amount of it evaporates into the atmosphere in some cases or under some conditions. The vast majority stays within the soils. Depending on the chemical makeup of the soil, the halogen can recombine with certain chemicals to form an even more poisonous chemical including Dioxin.

It has a predisposition of combining with sodium or potassium. Since neither of these is generally present, the herbicide simply adheres to the existing soil. The higher the ratio of surface area to volume of the soil particles the greater will be the adhesion and therefore the greater the underground lifetime of the herbicide. I have documented its continuation in highly granular soils for fifteen years, in sufficient quantity to be lethal to plants.

The herbicide will accumulate at or slightly above the water table. Every time the water table rises, there will be an additional discharge of herbicides into springs, streams and water bodies. This has occurred repeatedly over fifteen years (1972-1987) in the documentation. In this special case the ground surface lies on a slope of ten degrees, but the water table lies on a flatter slope of only two degrees. Ten years after the last application of herbicide on only the roadside ditches, it required a rainfall of one inch in twenty-four hours to begin the transport anew. At that rate of rainfall and that rate of slope, some of herbicides penetrated more than forty feet in depth. It required exactly eight days for the herbicide to be transported 700 feet underground, or a little less than one hundred feet per day.

The above is documented, repeatedly, over a span of fifteen years. The ground in question is a relatively dry alluvial fan. The kill of ground cover from trees to bushes shows clearly on aerial photographs as a strip of land which is one hundred to two

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hundred feet wide and one thousand feet long, with side branches at various places. The deeper rooted plants, mainly deciduous trees, were killed at the upstream extremity. This progresses downstream to where the springs break out at approximately 800 feet. From that point on downstream only annual plants can grow from repeated reseeding.

SYMPTOMS

Some of the symptoms of animal poisoning were given above. The first symptom of poisoning in plants comes from those which are flowering plants including fruit trees. They will break into excessive blooming and will continue blooming before and after unexposed similar plants. These excessive blooms however, are sterile and will not produce seeds or fruit. Nut trees such as hazel nut will produce great quantities of nuts, all with empty shells. Pomes, (apples and pears), will produce fruit to a limited extent but the fruit is deformed beyond recognition and is exceedingly small. Cherries will produce fruit which consists of shells and skin only, and no seed within the seed shell. There is a gradual disappearance of birds and animals. Birds will build nests for a few years, but the eggs do not hatch normally. Those which do hatch are often deformed. In this connection, 80 duck eggs were set for hatching. Of these, only six ducklings hatched. They were deformed and lived less than one week. It is calculated the rate of poisoning was on the order of only a few parts per trillion but was concentrated in the food chain. Laboratory tests were unable to detect the herbicide, possibly because the transport of the herbicide was unpredictable and samples were not taken during a short period of active transport.

There is no question that herbicides were accountable for the massive slaughter. Within twenty-four hours of the first rain after application of the herbicide all fish disappeared from a small stream, a disappearance of thousands of young fish. No other chemicals were used during the past fifteen years except the herbicide and the upland is all prime forest land. Subsequently, this specific stream died and had no organisms except algae and slime for eight of the fifteen years of record keeping. In the winter of 1986-87 three fish had spawning beds in this stream. From these three beds there resulted only three minnows to continue the species, and there is little feed in the stream for them. No crayfish, no frogs, no other smaller organisms. It has been a full ten years since the last herbicide application.

Prior to the herbicide application wild natural game was plentiful: chipmunk, squirrel, 5 black bear, deer, coyotes, cougar, raccoon, turtles, skunk, frogs, opossum, wildcat were all present. Now fifteen years later there is no animal track to

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be found anywhere, except the coyotes which barely managed to survive. There were 22 varieties of birds, but they all virtually disappeared and have only begun to resurge in numbers in 1987. The insect population practically disappeared. Bumble bees are presently scarce and honey bees are non-existent. The taking of water samples from streams is undependable for testing for presence of herbicide. A more simple method of testing for herbicide is taking a count of organisms in a stream on a daily basis. This will not only confirm the activity of the herbicide but will also disclose its lethality. The transport of herbicides in streams cannot be identified by taking water samples because of the sporadic nature of the transport. The active transport may last only a few hours and if sampling is not performed during that critical period the test results will always be negative.

CONCLUSION

These documented observations will show many of the statements in the EIS to be in error. Many of the above observations made, concur with those found in the testing laboratories, but the conclusion that "it is good" that herbicides will cause animals to gain weight is obviously a wrong conclusion. If these missing data are added in, the conclusions become reversed from what is outlined by the EIS.

The lifetime activity of herbicides can be taken to be the same length of time as its shelf-life in storage. The lethality of herbicide is present in any concentration what-so-ever because of its obvious concentration as it travels upward in the food chain in fats which are consumed. The LD50 rating of herbicides has no practical meaning. Its storage within the body is not essentially harmful in small quantities, but its absorption is cumulative and can quickly become lethal fifty years later when those stored fats are required by the body metabolism, due to strenuous activity or exposure to cold. When such a call is made by the body, it starts a cascade process in which will certainly bring death "from unknown causes" unless it is halted.

Within my documentation is such an example which occurred to me personally in 1981. The acne was on the external skin but also internal abscess deposits were found on x-ray. There was no known cure until I accidentally stumbled upon a useable antidote. There is little basic difference between herbicide poisoning from halogenated hydrocarbons and poisoning by carbon tetrachloride, or trichloroethylene. All will attack the liver and certain other organs. Each different halogenated hydrocarbon will additionally attack other selective organs as well. Simazine is a nerve poison, and herbicides made with iodine will attack the thyroid. Minor chronic poisoning suddenly turns acute, and it is often too

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late to act.

Because all halogenated herbicides are accumulative over time in body tissues, (and in the environment), there is no acceptable rate at which it can be entered into the environment.

NOTE: In the Personal documentation upon which this response is based the word "herbicide" is Generalized because the brand of herbicide was unavailable. On trying to determine the type of herbicide, or even the date applied, it was found that even the applicator could not supply this information because different herbicides were mixed together in a single tank loading. Dates of application and limits of application on any given date could not be determined because operational details were falsified on reports. Date and limits of application were established by daily observation of the stage of death for the affected plants.

- Ben Iverson
30211 S.E. 40th St.
Fall City Wash
98024

Response Form

To be most helpful, we need your concise and thoughtful comments on:

- ... The alternatives; telling us which ones you support, or changes that could be made.
- ... Our scientific information and analysis.
- ... The measures we have proposed to protect the environment.

- (1) It impacts the alternatives which are absolutely
no better - even in practice.
- (2) Your scientific evaluation is completely political
and economic, and non scientific. Your inference
are absolute.
- (3) Measures to protect the environment are
insufficient.
- (4) Your cost estimates ignore the economic benefit
of doing absolutely nothing.
- SEE ATTACHED 6 sheets to developing
information on hearings.

Ben Sherman
3024 SE 40th St
Fall City, Wash 98024

000418



January 6, 1988

Jim Torrence, Regional Forester
USDA-Forest Service
PO Box 3623
Portland, Oregon 97208

Dear Mr. Torrence:

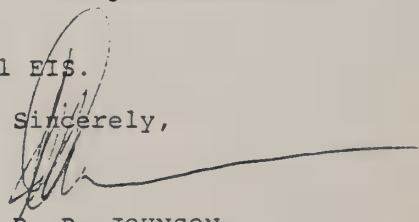
I would like to comment on proposed Vegetation Management DEIS.

I feel this is another attempt to cut the federal timber sale program. So, for the protection of our employees, our company and community, D.R. Johnson supports the alternative B-plus developed by Oregonians for Food and Shelter.

We as an Industry, have compromised enough with the preservationalists and now have our back against the wall.

Please accept alternative B-plus in your final EIS.

Sincerely,



D. R. JOHNSON,
President/General Mgr.



JOHNSON
LUMBER COMPANY



000418

December 28, 1987

James Torrence
Regional Forester
Forest Service
319 SW Pine
Box 3623
Portland, OR 97208

RE: DRAFT EIS MANAGING COMPETING AND UNWANTED VEGETATION

Dear Mr. Torrence:

D.R. Johnson Lumber Company operates a sawmill employing 100 people in Riddle, Oregon. In addition to this sawmill operation, we own and manage timber lands adjacent to National Forest Lands. The future of our company is dependent upon the prudent management of the National Forests, therefore we are submitting these comments for incorporation into the record.

Of major concern to our company is the impact the DEIS strategies may have on the timber supply long term sustained yield currently being developed in the Forest Planning process. Our review of a number of the draft plans reveals the LTSY projections in the plans are dependent upon the availability of a full range of vegetation management tools. Therefore it is our position that to insure the long term stability of our company and community that the Forest Service select Alternative B.

As an adjoining landowner to the National Forests, we are acutely concerned over the impacts on our land management practices when the Forest Service fails to control noxious and unwanted vegetation on its land. The Forest Service's failure to adequately control these noxious weeds increases our management costs and reduces the productivity of our lands. These are very real economic and environmental effects yet the DEIS fails to discuss this issue.

The DEIS on p. S-15 notes slash will not be piled in flood plains. This implies a change in current slash disposal techniques. If it is indeed a change additional discussion and explanation of its application is required.

P. O. BOX 66

RIDDLE, OREGON 97469



TELEPHONE (503) 874-2231

The DEIS appears to not only be covering a herbicide management program but also a new slash disposal program. If this program is reviewing the slash disposal program there should be a discussion of the use of cogeneration facilities as a means to dispose of the slash. Included in this discussion would be a cost analysis incorporating not only the cost to transport the slash but also the true costs of fire suppression, wildfires, and slash piling.

We strongly concur with the provisions in Alternative B which specifies action will occur when there is the first sign of potentially damaging competition. The Forest Service's failure to react promptly to insect and disease outbreaks in Oregon clearly demonstrates the necessity of prompt action.

It is difficult to believe the assumption that the Forest Service's budget would decrease if herbicides were not used. If a manual labor program is adopted in place of herbicides, to treat the same amount of acreages would be considerably more expensive.

The DEIS discusses the use of host-specific insects as a mechanism to control unwanted vegetation. Absent from this discussion is the impact the insecticides used to control such pests as Spruce Budworm may have on these introduced insects (ie. cinnabar moth). These conflicting management practices should be discussed.

The DEIS fails to discuss the impacts of unwanted and noxious weeds located on wilderness lands may have on general Forest lands. There should be a discussion of the impacts from these reservoir sites, and the application of management techniques to the wilderness areas to control those plants having off-wilderness impacts.

There is only limited discussion of insect and disease control programs. Either this DEIS should be expanded to cover insect and disease control programs or an entirely new EIS should be prepared addressing this problem.

The DEIS discusses past herbicide usage however absent from this discussion is a review of the adverse impacts encountered. This information is relevant in placing the risks in a true perspective.

One of the major problems in utilizing laboratory results to determine the environmental toxicity of a compound, is the failure of most laboratory protocols to duplicate actual field conditions. Few toxicology studies apply the herbicides in the same quantity or conditions as field applications. Therefore a careful review of the scientific methodology utilized should be done before studies are used as references. This review was clearly absent in the development of this DEIS.

The report indicates that herbicide degradation is usually biological in nature. This is generally true however a significant amount occurs from U-V light (ie. TCDD) and should therefore be discussed.

The discussion on p 1V-94 relative to 2,4-D and dioxin (TCDD) is too sketchy. This section could be expanded to demonstrate clearly that the manufacture of 2,4-D can not result in the creation of TCDD. To place the TCDD issue in proper perspective, reference should be made to the TCDD present in everyday products such as cosmetics.

The interjection of the TCDD issue into a 2,4,-D discussion raises a question as to the scientific accuracy of the publications relied upon. The manufacture of 2,4-D does not produce TCDD.

The discussion on 1V-116 notes that "...based upon suggestive, reproductive, ..." effects, 2,4-D is considered a herbicide of particular concern. With the confusion in the earlier discussions of 2,4-D and TCDD it is likely this statement is unsupported. Since TCDD is traditionally considered a teratogen, it is likely the authors are confusing 2,4-D with 2,4,5-T, or are referring to Agent Orange.

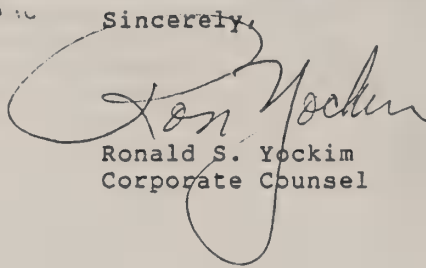
The horizontal buffer strips required around lakes and wetlands is an arbitrary decision. Since not all herbicides are toxic to aquatic organisms, it may be acceptable for some herbicides to spray without a buffer strip.

The DEIS does not discuss how the Forest Service will process the thousands of acres which were not treated while the injunction was in place. Since this will have a major economic impact the DEIS should discuss this issue.

The DEIS contains only a sketchy review of the literature available on the various herbicides. Therefore we suggest that

the DEIS undergo a peer review prior to final publication.

Sincerely,



Ronald S. Yockim
Corporate Counsel

RSY/sb

LAKEVIEW WATER USERS, INCORPORATED

HC 60 - BOX 2980

LAKEVIEW, OREGON 97630

(503) 947-3003



000415

December 29, 1987

Gary Larsen
Vegetation Management Group Leader
USDA Forest Service, Pacific Northwest Region
Portland, Oregon 97208

Subject: Draft EIS "Managing Competing and Unwanted Vegetation"

Dear Mr. Larsen

Enclosed is my response to the Draft EIS "Managing Competing Vegetation, I sincerely hope that it will be of some help to you.

The Lakeview Water Users, Incorporated is a farmer owned irrigation company, representing 136 stockholders with water rights for 11,514 acres of land. The water is supplied by two reservoirs located in the Fremont National Forest. The presents of noxious weeds in the watersheds of these two reservoirs are of great concern to us.

Sincerely

Herb Jessen
Manager

DRAFT RESPONSE

Managing Competing and Unwanted Vegetation Draft Environmental
Impact Statement

I have reviewed the Summary of the Draft Environmental Impact Statement "Managing Competing and Unwanted Vegetation. I was impressed with the amount of factual information, the alternatives and the fact that all alternatives are environmentally acceptable and achievable. I was concerned however that the summary did not address the impact that noxious weeds have on agriculture if not managed and controlled on the Fremont National Forest Lands, and are allowed to be transported.

HERBICIDIES

I reject all alternatives that restrict the proper use of herbicides. Herbicides like other pesticides can help the environment when they are used carefully and wisely. Proper training and licensing of personnel who would be handling and applying chemicals would lessen the risks involved. I conclude that any restrictions on land management activities do to the use of herbicides is not warranted.

ECONOMICS

I am concerned about the economic impact that noxious weeds have on agriculture in Eastern Oregon, particularly Lake County, if these weeds are left uncontrolled on the Fremont National Forest Lands. Noxious weeds cost Oregonians millions of dollars each year in crop losses and control measures. It is a fruitless effort for the rancher and farmer to attempt to control unwanted vegetation if this vegetation is allowed to remain uncontrolled in the watersheds of the National Forests.

Seed from noxious weeds are transported from the watersheds by wind, wildlife and water runoff and eventually end up in irrigation systems, on cropland and pastures. It is of the utmost importance that the Forest Service participate with State and private landowners in controlling unwanted vegetation.

After reading the Summary of the Draft and comparing the alternatives it appears that the only difference between alternatives B and G is the amount of commercial forest land that will be managed and the amount of herbicide treatment. Both B and G use all effective and efficient techniques of managing competing and unwanted vegetation. Both are also cost-efficient.

draft response cont.

ALTERNATIVES

- A. WITH NO HERBICIDES
- B. WITH ALL EFFECTIVE TOOLS
- C. RARELY, AND ONLY FOR HUMAN SAFETY
- D. EMPHASIZING PREVENTION AND THE USE OF NATURAL PROCESSES
- F. WITH NO BURNING FOR SILVICULTURE
- G. AGGRESSIVELY, WITH ALL TOOLS (LOOKING FOR OPPORTUNITIES)

CONCLUSION

1. From an economic point of view alternative G is the best alternative for the Region and Lake County. It provides opportunities to increase timber and forage production (through vegetation management), aggressively attack those noxious weeds which contaminate cropland and pastures and would provide for additional employment. Alternative G would not harm Lake County as would other listed alternatives.

2. Alternatives should be selected by sub-region rather than region as a whole. Each sub-region has its own unique issues and problems that have to be dealt with individually. An alternative that might be well suited for one sub-region might not be suited for another sub-region.

Lilly

001051

Lilly Research Laboratories

A Division of Eli Lilly and Company

Greenfield Laboratories
P.O. Box 708
Greenfield, Indiana 46140
(317) 467-4000

Mr. Gary L. Larsen
Vegetation Management
Group Leader
USDA Forest Service
Pacific Northwest Region
319 SW Pine, Box 3623
Portland, Oregon 97208

**RESPONSE TO USDA FOREST SERVICE DRAFT ENVIRONMENTAL IMPACT STATEMENT
ENTITLED "MANAGING COMPETING AND UNWANTED VEGETATION"**

The following comments are provided on the toxicology review of
tebuthiuron.

NEUROTOXICITY:

Statements on pages IV-103 and H-122 in Appendix H, Section 5, entitled, Data for Analysis of Immunotoxicity and Neurotoxicity, suggest that tebuthiuron caused neurotoxicity in the mouse, rat, cat and dog in acute studies. Signs of toxicity that were cited on page H-122 include hyper-irritability, loss of sighting reflex (which should be correctly described as righting reflex), ataxia, emesis, tremors, and convulsions. These signs of toxicity were observed at lethal doses of tebuthiuron, and are common in dying animals in which the normal metabolic pathways have been completely overwhelmed by exaggerated doses of chemicals in order to determine LD₅₀ values. An extensive tebuthiuron toxicology data base derived from several one and two year chronic studies to evaluate reproductive toxicity, teratogenicity and oncogenicity in mice, rats, and dogs has shown no gross or microscopic evidence of neurologic deficit, neuropathy or neurotoxicologic effects. It is totally misleading and unfounded to suggest that tebuthiuron is a neurotoxic herbicide.

MUTAGENICITY:

To supplement the mutagenicity evaluation of tebuthiuron given in Appendix D, page 17, and Appendix H, page H-76, please note the following studies that have been conducted and submitted to the U.S. EPA:

- a) Bacterial Mutation Studies: 840410 GPA 655
840326 AMS 655
- b) Hepatocyte DNA Repair Studies: 840503 UDS 655
840508 UDS 655
- c) L5178Y TK Forward Mutation Studies: 840410 MLA 655
840606 MLA 655
840613 MLA 655
- d) Sister Chromatid Exchange Study: 840710 SCE 655

The collective findings of this extensive battery of genetic toxicology tests support the conclusion that tebuthiuron has no intrinsic genotoxic potential. This conclusion is consistent with the absence of a carcinogenic effect in both rats and mice that received diets containing 400, 800, or 1600 ppm of tebuthiuron for two years.

REPRODUCTIVE TOXICITY AND TERATOLOGY:

In rebuttal to information contained in Appendix H, page H-98, please note that the reproductive capacity of rats fed dietary concentrations of technical tebuthiuron as high as 56 mg/kg/day was unimpaired through three successive generations and no abnormalities were detected in parents or offspring.

In a rat teratology study where tebuthiuron was administered in the diet at doses as high as 0.18%, there was no evidence of embryo/fetal toxicity or teratogenicity. Maternally toxic effects observed at 0.18% included depression of body weight gain and food consumption. No maternal effects were observed at 0.12%.

Based on these findings, tebuthiuron is not considered to be either a teratogen or a reproductive hazard.

David S. Negilski, M.S.
Assistant Senior Toxicologist

Response Form

004952

To be most helpful, we need your concise and thoughtful comments on:

- ... The alternatives: telling us which ones you support, or changes that could be made.
- ... Our scientific information and analysis.
- ... The measures we have proposed to protect the environment.

The Longview Chapter of the Society of American Foresters, upon reviewing the Draft Environmental Impact Statement for Managing Competing and Unwanted Vegetation, supports Alternative B. We support this Alternative for several reasons. First, we believe that all tools should be available so that silviculturists can select the best tool or combination of tools to treat any given situation. Second, we agree that prevention is preferable to correction as the latter indicates that a loss may have already occurred. Third, Alternative B allows the use of prescribed burning and herbicides where and when appropriate. This also makes available other types of vegetation control for those areas that are more "sensitive" for whatever reason. Fourth, with the exception of Alternative "C" there seems to be an insignificant difference in risk between the other alternatives yet Alternative "B" has greater social and economic benefits than the other alternatives with the exception of "C" which we feel is too aggressive and wasteful. Fifth, with all tools available for use, we feel that there will be more emphasis placed on using economic analysis of costs and benefits to determine the correct prescription to control unwanted vegetation. We believe that under Alternative "D" the driving force would be "perceived risk" and not economics.

To conclude, it is our position that the Commercial Forest Land within the National Forest System that is designated for timber production should be managed aggressively to produce good yields of timber which will maximize the dollar return on the investment. We feel that doing other than managing these lands aggressively would adversely effect those who directly or indirectly derive their livelihood from public timber land production. We believe that the use of prescribed burning and herbicides when applied according to laws and labels does not pose a significant risk to workers or the public. This perceived risk is far outweighed, in our opinion, by the economic benefits to this region.

The wise use of all silvicultural tools backed up by economic analysis and risk assessment on a unit by unit basis will return to the public maximized social and economic benefits and minimal environmental adverse affects. We believe that Alternative "B" will give the tools and guidance necessary to do this.

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U.S.D.A. FOREST SERVICE
BEFORE THE PACIFIC NORTHWEST REGIONAL FORESTER

<u>In re:</u>	MANAGING COMPETING AND)	COMMENTS OF
	UNWANTED VEGETATION)	PAUL E. MERRELL
	DRAFT ENVIRONMENTAL)	and
	IMPACT STATEMENT)	CAROL VAN STRUM
)	

Paul E. Merrell and Carol Van Strum submit these comments on the above-captioned draft environmental impact statement ("DEIS").

INTRODUCTION AND TABLE OF CONTENTS

While claiming to turn over a new leaf, the Forest Service has prepared yet another environmental statement whitewashing the effects of its management practices. The DEIS unlawfully: (1) fails to disclose the Forest Service's proposed action; (2) fails to examine the possibility that far more significant cumulative impacts may occur beyond those acknowledged in the DEIS; (3) fails to comply with -- or even acknowledge the existence of -- court orders directed to the Forest Service in two major lawsuits; (4) substitutes conclusory and misleading statements and assumptions in lieu of necessary research or, alternatively, worst-case analysis; (5) fails to develop adequately the required

alternative of postponing the proposed uses of fire and herbicides pending further study; and, (6) fails to address substantive limits to the Forest Service's discretion in rejecting the alternative of postponement pending further study.

The wanton failure of this DEIS to acknowledge even the possibility of a risk to public health is perhaps best illustrated by the photographs below:



The photographs were taken in August, 1984, a few miles upstream from our house. The men in the pictures are EPA

officials, wearing decontamination suits, taking sediment samples for TCDD analyses from a tributary of the stream that flows through our farm. At the request of EPA, Ms. Van Strum guided these officials to the site near the old Mill Camp in Five Rivers where other EPA officials had gathered samples in the late 1970's. The site was a sump in Summers Creek, the potable water supply for the families that lived in Mill Camp, and analyses in the 1970's had showed hazardous levels of TCDD. In 1984, the EPA officials refused to drink water or coffee at our home, warned people not to wade in Summers Creek; afterward, one of the officials put her boots in a sealed container and said she would dispose of them if the samples proved positive, which they did, showing that levels of TCDD in Summers Creek sediments had increased nearly four-fold in five years.

This is only part of our toxic legacy from decades of Forest Service and timber industry spraying of TCDD-contaminated herbicides. Our family swims in Five Rivers almost daily during the summer months. We eat the fish from the river. Our farm animals drink from it. The deer, elk, and other wildlife drink from it.

For more than thirty years, the Forest Service sprayed TCDD-contaminated 2,4,5-T throughout Region VI. On March 1, 1979, EPA for the first time used its emergency powers to protect human health by banning 2,4,5-T in large part because of a study showing a statistical relationship between use of 2,4,5-T and involuntary

human abortions in a 1,600-square-mile area surrounding our home in the Siuslaw National Forest (the "Alsea Study").

That ban was preceded by years of Forest Service environmental impact statements concluding -- like the present DEIS -- that there was no possibility of a threat to public health from the use of herbicides. Yet the Alsea Study, the emergency ban, the years of spreading TCDD in the forests, none of this is even mentioned in the DEIS. No one in the Forest Service has looked to see why they might have arrived at the wrong conclusion before, or sought to determine what was wrong with the risk assessment methods used.

Once again, the Forest Service concludes that there is no danger to public health because it will expose the public only to low doses of its herbicides. In a thick volume of appendices stuffed with exponents, equations, authoritative references, and confident prose, the decisionmaker and the public are told that there is an adequate margin of safety for the public from this program using substances that admittedly may cause cancer, birth defects, and mutations at some dose. No scientific uncertainty is acknowledged.

William Ruckelshaus, however -- twice EPA Administrator and a long-time executive of the huge timber conglomerate Weyerhaeuser Corp. -- said in 1983:

"[W]e must now deal with a class of pollutants for which it is difficult, if not impossible, to establish a safe level. These pollutants interfere with genetic processes and are associated with the diseases we fear

most: cancer and reproductive disorders, including birth defects. The scientific consensus is that any exposure, however small, to a genetically active substance embodies some risk of an effect. Since these substances are widespread in the environment, and since we can detect them down to very low levels, we must assume that life now takes place in a minefield of risks from hundreds, perhaps thousands, of substances. We can no longer tell the public that they have an adequate margin of safety."

W. Ruckelshaus, Science, Risk, & Public Policy, 221 Science 1026, 1027 (September 9, 1983); Appendices Vol. I, pg. 203.

The dissonance between the conclusions of the DEIS and what Mr. Ruckelshaus labels as scientific consensus suggests that some fundamental truth was overlooked by the drafters of the DEIS. It is as though science has evolved for decades, with no notice taken by the Forest Service.

Indeed, it is not only science that has been ignored. There is not a single mention in the DEIS of two major lawsuits we have won against the Forest Service over years of full-time effort to avoid being poisoned by our government. Busy federal judges have in both cases devoted disproportionate care in instructing the Forest Service what it must do to comply with the law, but to no avail. Their opinions simply have been ignored. And, there is clear evidence of bad faith in the preparation of the DEIS. This is discouraging, not just because it is unlawful but also because it smacks of disrespect for the law, for the established means of resolving honest disputes peacefully. It suggests a belief or a hope that judges will change their minds and soften their decrees

if asked often enough, regardless of what they ruled before.

We are also particularly disturbed by the DEIS's failure to come to grips with the cumulative impact issue. There is strong evidence that human beings in the affected area already are well past any supposed "threshold level" for toxic effects. Our concerns in this regard were deepened on January 7, 1988, when our youngest son, Nicholas Merrell, was born with a serious congenital hydronephrosis of his right kidney's urethra, a birth defect produced by low-level TCDD exposure in animal laboratory experiments. Nicholas has already had to undergo painful tests and faces a strong possibility of corrective surgery within a year. We can only wonder what other more subtle defects have gone unnoticed, and worry over even more frightening possibilities such as a latent cancer triggered by the cancer promoting-dioxins and any other inadequately-tested chemicals introduced into his body by his own government.

This is our reality, and we can only hope that those who read these comments will understand both why we are fearful and why we are angered. The lack of detail in the DEIS is appalling and violates both common sense and the law. What is involved in this program is a question of human death, birth defects, mutations, and other serious health effects, not a question of how to construct oversimplified mathematical models that have no relevance to the real world. We are dealing with a myriad of molecules to be introduced into a world of other molecules, and only the ignorant and the dishonest can find scientific certainty

in claims of safety.

The molecular landscape of Region VI is a garbage dump. The safety of still other toxic chemicals to be added to that molecular garbage dump is, as Mr. Ruckelshaus tells us, difficult if not impossible to predict; but if safety is to be predicted even roughly, enormous detail is required, as is a frank admission of scientific uncertainty. For example, EPA's examination of 2,4,5-T in its cancellation proceedings produced a record of over 20,000 pages of transcript alone, without ever considering the herbicide's benefits. Even after such a major effort, safety could not be established.

If the Forest Service "is to go ahead with the project in the face of these uncertainties, at least it must do so knowing of the fate to which it may be condemning future generations." S.O.S./Merrell v. Clark, 747 F.2d 1240, 1246 n. 9 (9th Cir. 1984).

Those future generations are not faceless statistics; they are our children.

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Threats to the health of the future generations must -- because of basic instincts of humanity -- be examined closely. Government programs involving potential adverse human health effects are therefore understandably at the zenith of the law's requirement for detail. The required detail in an EIS depends upon the nature and scope of the proposed action. California v. Block, 690 F.2d 753, 761 (9th Cir. 1982). An EIS must be particularly thorough -- and accurate -- when the potential

consequences involve a threat to human life. Warm Springs Dam Task Force v. Gribble, 621 F.2d 1017 (9th Cir. 1980). No subject to be covered by an EIS can be more important than the potential effects of a federal program upon the health of human beings. Citizens Against Toxic Sprays v. Bergland, 428 F. Supp. 908, 927 (D. Oregon 1977). The Forest Service may not rely upon forecasting difficulties or the task's magnitude to excuse the absence of a reasonably thorough site-specific analysis of the decision's environmental consequences. California v. Block, 690 F.2d at 765. Agencies must, as a matter of law, be capable of compliance in terms of personnel and other resources. 40 C.F.R. 1507.2. An agency is not allowed to prepare inadequate DEIS's and cure defects in the final EIS. 40 C.F.R. 1502.9(a) (DEIS must fulfill and satisfy to the fullest extent possible the ^{1/} requirements established for final EIS's) (emphasis added).

This DEIS is unacceptable. Because the DEIS drafters have repeatedly omitted or arbitrarily truncated required discussions and information, we are unable to prepare meaningful comments on the omitted subjects. This handicap has been exacerbated by the Forest Service's failure to provide requested information

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See also 40 C.F.R. 1500.6 (phrase "to the fullest extent possible" means agencies "shall comply" unless existing law "expressly prohibits or makes compliance impossible." This obligation of strict compliance is not excused except for "trivial violation." 40 C.F.R. 1500.3; see also Oregon Environmental Council v. Kunzman (II), 817 F.2d 484, 492 (9th Cir. 1987) (courts will only ignore "fly speck" deficiencies that are inconsequential or technical).

underlying the DEIS within the time provided for public comment. The lack of care exercised by the DEIS's drafters -- either alone or in combination with evidence of their bad faith in considering potential health and environmental effects -- impedes public participation and makes the entire document untrustworthy as a basis for agency decisionmaking. The omission of required information and discussions makes this DEIS "so inadequate as to preclude meaningful analysis." 40 C.F.R. 1502.9(a). A new or supplemental DEIS must therefore be prepared. Id.; California v. Block, 690 F. at 769-72.

To the extent that the Forest Service regards any information included in or with these comments as "new" information, we request that a revised or supplemental DEIS be prepared because of the significance of that information.^{2/}

Alternatively, any substantive decision to use herbicides or prescribed fire despite the substantial risks to human health would be unlawful under 5 U.S.C. 706(2)(A). Gribble, 621 F.2d at 1027; see also id. at 1026 ("Any substantial risk . . . would be

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Gribble, 621 F.2d at 1023-27 (when new information comes to light, agency must consider it, evaluate it, and make a "reasoned" determination whether "significance" of information requires supplementation); 40 C.F.R. 1502.9(c); Oregon Natural Resources Council v. Marsh, 820 F.2d 1051, 1056 (9th Cir. 1987) (adv. sht.). See also 40 C.F.R. 1502.2(b) (evaluation of significance must include in EIS "enough discussion to show why more study is not warranted"); S.O.S. v. Clark, 747 F.2d 1240, 1243-44 (9th Cir. 1984) (evaluation of significance requires worst-case analysis); Gribble, 621 F.2d at 1027 (now-Justice Kennedy observing that 9th Circuit interpretation of supplementation regulations "finds support" in "analogous" worst-case analysis regulation).

intolerable; and, if the agency were to proceed in the face of that risk, that would constitute an abuse of agency discretion").

The substantive decision to proceed despite the risks would also create a substantial and imminent danger to public health unlawful under the Resource Conservation & Recovery Act, 42 U.S.C. 6972(B), and would create either a nuisance or a trespass actionable at common law.

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U.S.D.A. FOREST SERVICE
BEFORE THE PACIFIC NORTHWEST REGIONAL FORESTER

In re:	MANAGING COMPETING AND)	
	UNWANTED VEGETATION)	COMMENTS OF
	DRAFT ENVIRONMENTAL)	PAUL E. MERRELL
	IMPACT STATEMENT)	and
)	CAROL VAN STRUM

I. BACKGROUND

Paul E. Merrell and Carol Van Strum are inholders, residing on a small farm within the U.S.D.A. Siuslaw National Forest, whose health and environment has been and will be affected by the vegetation management program at issue. They live with their children in the Five Rivers Valley of western Oregon, along a major tributary of the Alsea River, surrounded by National Forest System lands.

Both Mr. Merrell and Ms. Van Strum have successfully litigated the inadequacy of environmental documents purporting to

justify portions of the "program" at issue. Van Strum and her neighbors obtained injunctive relief against part of this program in Citizens Against Toxic Sprays v. Bergland, 428 F. Supp. 908 (D. Oregon 1977). The injunction barred continued use of dioxin-contaminated herbicides until the Forest Service prepared an adequate environmental impact statement detailing the human health effects of dioxin pollution. Mr. Merrell subsequently obtained an injunction against continued use of all herbicides in the Siuslaw National Forest in Save Our ecoSystems/Merrell v. Clark, 747 F.2d 1240 (9th Cir. 1984) ("S.O.S./Merrell"). Herbicides were completely banned because of "widespread fraud" in the tests relied upon by the Forest Service in their last EIS, until the Forest Service prepared a new EIS including the results of basic research on the health effects of the herbicides.

Forest Service spraying has been at a standstill in the Siuslaw National Forest since the latter decision. The Forest Service, however, chose to ignore the S.O.S./Merrell decisions in the remaining portions of Region VI, even though the programmatic EIS had been found invalid, necessitating a further lawsuit to halt the remaining portions of the program. See Northwest Coalition for Alternatives to Pesticides v. Block, Civil No. 83-6272 (D. Oregon, Opinion of January 6, 1984) (enjoining all use of herbicides by the Forest Service in Region VI and by the Bureau of Land Management in Oregon).

The Forest Service's subsequent efforts can be accurately

characterized as an effort to escape compliance rather than to comply with judicial decisions. The Forest Service, along with other federal agencies and private organizations and individuals, has sought to circumvent the Ninth Circuit herbicide case decisions on varied fronts. For example, the Forest Service made a strong effort to obtain amendment of 40 C.F.R. 1502.22, on which the decisions rest in part, before the President's Council on Environmental Quality (further discussed below).

The Forest Service also published amendments to its regulations implementing NEPA, categorically excluding from NEPA procedures all site-specific decisions to apply herbicides by methods other than aerial application. See 49 Fed. Reg. 37306 (September 21, 1984).

The Forest Service's coalition also sought legislative reversal of the Ninth Circuit herbicide decisions through an eleventh-hour amendment to a bill amending the U.S. pesticide regulation law, the Federal Insecticide, Fungicide & Rodenticide Act. Shortly before the 99th Congress adjourned, the Forest Service's amendment (S. 2705) -- already defeated in Committee -- was added to amendments to the pesticide law on the floor, causing the failure of the entire bill. The Forest Service's amendment would have excused the Agency entirely from considering human health effects of pesticides when preparing environmental impact statements. Paul Merrell coordinated the lobbying that defeated the legislation. He was repeatedly told by House Agriculture Committee staffer Nick Ashmore, the principal

staff backer of the last-minute amendment, that the Forest Service and Bureau of Land Management were the driving forces behind the amendment. The Forest Service had never prepared an EIS on its legislative proposal, as required by 42 U.S.C. 4332(2)(C).^{1/}

With its unmistakable goal of changing or evading rather than complying with the law, it is not surprising that the relevant DEIS -- under preparation during the same period -- is legally and factually deficient.

II. CRITICISM

U.S.D.A. Forest Service has a bad track record in complying with the National Environmental Policy Act of 1969. The Agency has lost far more NEPA cases in court than it has won in the last decade. The following discussion explains how the Forest Service becomes so blinded to the potential human health effects of its land management program. It also explains why this DEIS must be withdrawn and site-specific EIS's must be prepared before the Forest Service can lawfully conduct any further timber sales on the assumption that standing timber will be replaced through future use of herbicides: The Forest Service has created such a procedural morass that no other actions can restore the agency to lawful conduct.

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A brief history of this debacle from the Journal of Pesticide Reform is included in Appendices, pp. 1-3.

A. WHY THE FOREST SERVICE DOESN'T COMPLY WITH NEPA

NEPA is more than just another law affecting social relationships; it is a planning and decision-making process. Section 102(2)(C) mandates, in the plainest of terms, that a final environmental impact statement will have been prepared by the time an agency proposes actions that may have a significant effect on the environment. The Agency is required by the time such actions have been proposed already to have considered alternative courses of action, to have taken comments from other agencies and the public, and to have disclosed all significant environmental impacts of each alternative including the proposed action.

The fundamental issue faced by a land management agency such as the Forest Service is how to manage public land, the process of stewardship. If the goal is to produce a variety of outputs from a given unit of land while protecting the land and the public from harm, logic suggests that the beginning point for environmental analysis should be the land unit itself, its environment, its productive capacity, its existing relationship to society, the proposed alterations. Suggested additions to or subtractions from the existing land unit can be projected with the land as the beginning point. The basic wisdom or folly of alternative courses of action can be assessed when site specific actions are proposed.

This, however, is where the Forest Service first runs afoul of NEPA, because the Forest Service ends rather than begins its decision-making process at the site-specific level of management.

Target resource output levels are set at the national level and disbursed to the Regions as regional targets or quotas. Each Region has its quota of board feet of lumber, tons of mineral ore, acres of wilderness, and other resources that must be extracted. The Region breaks up the target figures and assigns quotas to the Forests, which in turn allocate target resource extraction levels to the Districts. At the very bottom of the heap, the land unit managers have very little discretion to manage the land according to the unit's optimum potential. They are constrained -- if they would advance their careers -- by the very nature of the top-down process used by the Forest Service to meet the resource extraction target levels. Their only real consideration can be how to meet the resource extraction targets, rather than to consider what level of resource extraction can be sustained without unacceptable environmental effects.

The problem is that in such a top-down decision-making process, the needs of the land and the people who live near it become secondary. "Getting Out The Cut" ("GOTC") is far more measurable and important in the Forest Service's decisionmaking process than GOTC's aftermath. The real decision -- the resource extraction decision -- is made in Washington, D.C. years before rural Oregon residents are told that as an inevitable consequence forest land will be clearcut up to their property boundaries, that the slash will be burned to fill the valleys with noxious fumes and smoke for weeks of the precious sunny days every year, that

the streams through their farms will be turned to mud and silt, that helicopters will pollute the air and watersheds with herbicides and fertilizers, but that none of this will significantly affect their environment, and a site-specific environmental impact statement is therefore unnecessary. Such findings of no significant impact violate common sense, insult the awareness and intelligence of the citizenry, provoke lawsuits, and invite court injunctions.

Indeed, the very goal of Forest Service land management activities is to significantly affect the forest environment, maximizing the resource output from every unit in the National Forest System. With resource extraction decisions already made, however, NEPA becomes a merely post hoc paperwork exercise, rather than a planning process. The fundamental principle of environmental management -- basing study and decision-making on the affected environment -- has been lost. Site-specific environmental impact statements are not prepared on the original GOTC decision, only on how to meet the resource extraction target.

Under such a regime, NEPA's mandate to assess alternatives and impacts before making decisions receives only lip service, because the decision that drives all subsequent activities has already been made. Furthermore, because site-specific decisionmakers are in reality implementing only portions of a decision already made, the Forest Service environmental analysis process becomes piecemeal.

The present programmatic DEIS is an obvious example of this

problem of fragmented and deferred analysis. The DEIS is titled "Managing Competing and Unwanted Vegetation," but is patently misnomered. Forest Service Region VI does not have a program of "managing competing and unwanted vegetation." Nowhere in the Region will an office be found where such a program is administered. Indeed, the notion is ludicrous to anyone familiar with Forest Service practices.

The Forest Service does conduct a variety of activities that produce "competing and unwanted vegetation." Except following natural disasters, however, "competing and unwanted vegetation" is normally only an indirect environmental effect of other management activities. For example, clear-cut harvesting of trees creates ideal conditions for emergence of "competing and unwanted vegetation." But the notion of managing "competing and unwanted vegetation" as a separate program is as preposterous as saying that weeding a garden is unrelated to growing a garden. The Forest Service does not have one program for harvesting and growing crop trees and another program for controlling the "weed" trees growing in their midst. Neither does it have one program for building and maintaining roads and a separate program for controlling the vegetation growing alongside the roads, nor does it have one program for facilities maintenance and another for controlling the weeds around the same facilities.

The point of this discussion is that rational environmental analysis must begin with the resource units -- the forest unit,

the road, the facility — and the proposed alterations to that unit; once the analysis has been conducted at that level, the sum of the impacts and resource outputs on many units can be analyzed, for example at the Forest level, then the Regional level, and finally decisionmakers in Washington, D.C. could be informed of their options and the options' likely and possible cumulative impacts.

The fundamental question is what the Forest Service's proposal actually is. "A proposal may exist in fact as well as by agency declaration that one exists." 40 C.F.R. 1508.23. Only after the proposed action is accurately defined can its effects and alternative courses of action be assessed. The critical threshold is reached where the agency proposes to make an "irreversible and irretrievable commitment of the availability of resources" to a project at a particular site. California v. Block, 690 F.2d at 761. Such commitments were already made in this case, by setting present resource extraction levels in the Siuslaw National Forest on the assumption that herbicides and fire will be used. See id. at 763. Where, as here, the "critical decision" to commit natural resources over the planning period has been made, that was the point at which a site-specific EIS disclosing site-specific impacts should already have been prepared, before the commitment is made. Id., 690 F.2d at 760-^{2/}65.

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Harvesting timber on the expectation that herbicides will be

Despite its title, this DEIS does not propose a distinct regional program of managing competing and unwanted vegetation. No regional review and approvals of site-specific plans are contemplated. "Mitigation measures" are only listed and are not made binding, as might have been demonstrated by inclusion in the Regional Guide. Acreage figures for particular management prescriptions expressed in the alternatives are subject to no regional constraints. (Forest Supervisors are not required, for example, to limit their use of herbicides or fire to acreage totals given in the EIS. The DEIS "worst-case analysis, moreover, covers a spray program of 100,000 acres annually, with no required

used without first assessing herbicide impacts is thus a straightforward violation of NEPA because the justification for the harvest is the proposed use of herbicides. This is particularly true when the use of herbicides has been enjoined.

"While work on a required program environmental impact statement is in progress and the action is not covered by an existing program statement, agencies shall not undertake in the interim any major Federal action covered by the program which may significantly affect the quality of the human environment unless such action:

- (1) Is justified independently of the program;
- (2) Is itself accompanied by an adequate environmental impact statement; and
- (3) Will not prejudice the ultimate decision on the program. Interim action prejudices the ultimate decision on the program when it tends to determine subsequent development or limit alternatives."

40 C.F.R. 1506.1(c) (emphases added).

regional reexamination of actual use levels no matter which preferred alternative is selected.) In short, the regional "program" is nothing more than a decision to get the herbicide issue off the Regional Forester's desk and onto the desks of the Forest Supervisors with a blank check to do anything they want.

Indeed, this DEIS describes only a hodge-podge of remedial management activities, all aimed at mitigating the secondary environmental impacts — unwanted vegetation — of other management decisions already made involving timber harvest, road construction, facility operation, overgrazing, etc.. These, however, are the man-made causes of "competing and unwanted vegetation." These are lawfully the proposals that are subject to NEPA. NEPA's focus is on the proposal that will cause environmental effects, not on how to cope with the environmental effects of decisions already made without full NEPA compliance.

The present DEIS therefore represents an admission that environmental effects of other management decisions were not adequately addressed in NEPA documents accompanying those decisions; i.e., that consideration of the environmental effects of silvicultural prescriptions already made, for example, were deferred to this EIS. In effect, the Forest Service asks the public to accept, for example, that the practice of clear-cutting is inevitable and the issue covered by this EIS is only how to deal with the resulting resurgence of "weed" species that naturally spring forth to renew the seral progression, flourishing on exposed clearcut sites.

The difference in focus, however, is critical. It is the difference between focusing on cause or effect, between focusing on a decision or its repercussions. It is the difference between treating a disease or only its symptoms.

For example, accepting clearcutting as inevitable is simply another facet of the Forest Service's approach to environmental problems that all too predictably leads to injunctions. Once clearcutting is accepted as a given, then "unwanted" vegetation is equally as predictable. "Unwanted" vegetation -- even though bared forest soils ordinarily require these pioneer species for soil stabilization and nutrient replenishment to set the stage for the next seral progression -- leads inexorably to increased costs and environmental impacts to remove the "unwanted" species by various means examined in the DEIS, such as herbicides and fire, which may have undesired effects on humans and wildlife. Noxious weeds invade the seedbeds created by bared soil. Removal of the unwanted species leads to further soil erosion and nutrient depletion, necessitating increased costs for slope stabilization and fertilization. Soon, an ecological imbalance causes even further impacts, such as laminated root rot, and the Forest Service incurs even further costs to replant alder seedlings to repel the pathogen. Even-aged management, or a harvest-oriented management regime, invites predation by forest pests such as the mountain beaver or "boomer", and by insects such as the gypsy moth or the spruce budworm. More costs are incurred for more sprays to

correct the latest imbalance, and additional toxic effects in humans and wildlife may result. The Band-Aid approach to environmental problems does not have a good track record, on the ground or in court.

It is far more enlightened and less expensive not to cause such wounds in the first place, rather than to plan on a lifetime spent in applying fresh bandages.

Similar mechanisms are at work with the design of roads, rights-of-way, facilities, and grazing plans. Unless the predictable costs and secondary impacts are factored into the initial management decision that produces unwanted vegetation, something has gone awry logically, fiscally, and legally. Suggesting that the proposal only involves the management of competing and unwanted vegetation is therefore far wide of the ^{3/} mark.

That this DEIS does not focus on the real proposals is evident from the fact that the effects of all non-polluting alternatives are expressed in terms of reduced resource outputs from those already planned. The allowable cut, for example, is

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See e.g., Thomas v. Peterson, 753 F.2d 754, 758-59 (9th Cir. 1985); 40 C.F.R. 1508.25(a)(1) (effects of connected actions must be considered together in a single EIS, agency discretion to define scope of EIS is limited); City of Tenakee Springs v. Block, 778 F.2d 1402, 1406-07 (9th Cir. 1985) (Forest Service discretion to define scope of actions does not it "to determine the specificity required by NEPA;" rejecting contention that Forest Service could avoid EIS on "whether to build roads and log," and limit EIS to decision on "how to do it") (emphasis in original). Compare the latter with the Forest Service's present procedure.

accepted as the status quo, even though the allowable cut was obviously set without consideration of the environmental impacts of the management practices (herbicides and prescribed fire) needed to sustain harvests at desired yields, and even though the use of herbicides has been the subject of court injunctions for the last five years.

Thus, the use of herbicides and fire has been proposed every time in the last few years that the Forest Service planned to use herbicides in setting allowable harvest levels under principles of sustained yield. Every addition of an "earned harvest factor" to the allowable harvest equation on the assumption that herbicides and fire would be used was a proposal to use these management prescriptions. With years of resources extracted in the meantime, the issue -- as seen by the Forest Service -- is reducing extraction levels to compensate for the resulting overharvest if herbicides and fire are not used.

By violating NEPA's requirement that environmental effects be assessed at the proposal stage rather than after the point when resources -- such as standing mature timber -- are irretrievably committed to the decision, the Forest Service has created incredible pressure to use the management prescriptions already selected. Future decisions concerning these planning units is constrained by the prior resource extraction decisions. See California v. Block, 690 F.2d at 762; Thomas v. Peterson, 753 F.2d at 760 (cumulative effects assessment of steps necessary to extract resources must precede resource extraction decision).

Timber companies, counties, and other vested interests have been alerted to the stakes. The drafters of this DEIS are thus forced to focus on mitigating impacts of decisions already made, to express resource effects in terms of reductions in outputs, and to attempt to justify the resource outputs already selected without serious consideration of environmental impacts. Predictably, the principal tool is a skewing of the real cost-benefit equation. "Benefits" of high resource production are hyped while the existence of costs such as environmental impacts are glossed over.

Precisely such practices led Congress to enact the National Forest Management Act of 1976, 16 U.S.C. 1600 et seq., establishing a "bottom-up" resource management planning process commencing at the unit level, which the Forest Service has violated either by increasing or maintaining artificially high harvest levels despite court injunction.^{4/} Now, the Forest Service proposes in this DEIS to examine the environmental effects

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See 16 U.S.C. 1604(f)(1) ("one integrated plan for each unit of the National Forest System"); 16 U.S.C. 1604(g)(1) (requiring compliance with NEPA in development of land management plans). The NFMA, 16 U.S.C. 1604(g)(3)(D) specifically prohibits the Forest Service from increasing harvest levels unless planned reforestation practices "justify" the increase. See also id. (requiring Forest Service to reduce harvest levels "if such practices cannot be successfully implemented . . . substantially as planned").

The Forest Service has blithely continued overharvesting based on planned use of herbicides, despite the fact that the use of herbicides can neither be justified nor successfully implemented because the use of herbicides has been enjoined for five years.

associated with a decision to reduce harvest levels, when the reduction was already mandated by the NFMA. 16 U.S.C.

1604(g)(3)(D). In other words, once again, the Forest Service process is diametrically opposite that required by law, because the agency has violated the NEPA "proposal" requirement.

In fact, the Forest-level land management plans prepared in the Region -- mandated by the NFMA -- state that they contain the actual proposals, e.g., use of herbicides and fire in silvicultural practices, road and trail maintenance, facilities maintenance, noxious weed control, etc., but that consideration of environmental effects of herbicide use have been deferred for consideration in this EIS. Now the public is being told in this DEIS to go back to the Forest Supervisors with their site-specific concerns. The Forests, however, are already telling the public they will not prepare site-specific EIS's, as demonstrated below.

Public review and comment requirements are not a game of ping-pong, and the "bureaucratic shuffle" does not meet the reasonable requirements of NEPA. The Forest Service can not escape its duty to prepare a site-specific EIS by abdicating responsibility from one office to another.

The logical conclusion is therefore unavoidable: This is not an environmental impact statement because it does not focus on a proposal for agency action. The Forest Service does not have a regional program or proposal to manage competing and unwanted vegetation. It is false for the Agency to suggest that it does. The Forest Service does, however, have programs and proposals that

cause "unwanted and competing vegetation." This EIS can only be intended to justify previous site-specific management decisions that were made without compliance with NEPA, without consideration of human and environmental effects at the proposal stage of the process.

Those previous decisions are unlawful and can not form a proper basis for preparing this EIS. This backwards approach to NEPA has been repeatedly used by the Forest Service. Small wonder that the Forest Service is rarely successful in defending its environmental impact statements in court, because they are sham and issued in bad faith.

B. BAD FAITH IN SELECTION OF PREFERRED ALTERNATIVES

Assuming in the alternative that the Forest Service does have a regional program of managing competing vegetation, the Forest Service has prepared this DEIS in bad faith. Numerous Forest Service documents demonstrate that the decisions to use herbicides and prescribed burning -- supposedly to be decided after this EIS is completed -- were actually made long ago.

1. BAD FAITH IN PREPARING THIS DEIS

A Forest Service internal document from the planning stages of this DEIS reveals that the DEIS comparison of alternatives is a sham and that an overt decision was made to write a justificatory EIS. See timeline for resumption of spray program, Appendices,

pp. 4-6, apparently authored by Gary Larsen, principle author of this DEIS. (Noting that "[n]ot using herbicides costs us more than \$8,000,000 per year" and that the record of decision will "reimplement" the previous vegetation management program, all under title of "summary of efforts to dissolve herbicide injunction.")

Predictably, therefore, a non-polluting alternative has not been seriously considered by the Forest Service. The Forest Service has fundamentally reversed the NEPA process; rather than examining alternative courses of action in good faith before making a decision, the Agency instead decided to use herbicides before issuing even a draft, and circulated this DEIS for public comment long after the decision had actually been made, as further discussed below. The public is unlawfully left in the posture of trying to persuade the Forest Service to change its decision, rather than being given a meaningful opportunity to participate in the decisionmaking process.

Bad faith has thus permeated the entire preparation of this DEIS. Under such circumstances, the Forest Service's request for comments from the public makes a mockery of the process envisioned by NEPA and the NFMA. Furthermore, in its efforts to conceal the fact that the decision has already been made, the Forest Service has set up an impenetrable smokescreen that makes it impossible for citizens to prepare informed comments.

2. FAILURE TO IDENTIFY PROPOSED ACTION IN EIS

The Regional program of managing competing and unwanted vegetation is a secret program being implemented far outside the disclosure provisions of NEPA. In other words, this is an EIS on a secret program, with secret impacts.

The EIS unlawfully identifies three preferred alternatives. Nowhere is the public advised what the Forest Service's proposed action actually is. This approach has already been rejected by the Ninth Circuit and is fatal to the DEIS.^{5/} As timber industry spokesman Mike Sullivan said of this DEIS,

"Those three preferred alternatives cover everything in the book. They are sufficiently ambiguous that the Forest Service is not put on the spot."

Appendices, Vol. I, pg. 15. Putting the agency's proposal on the spot, however, is precisely what the National Environmental Policy Act was intended to accomplish. The Forest Service can not have it both ways: either it has a proposal -- in which case the NEPA disclosure requirement is triggered -- or it does not yet have a proposal and it is too early to prepare an EIS. See Kleppe v. Sierra Club, 427 U.S. 390 (1976).

3. FAILURE TO REVEAL PROPOSAL IN RESPONSE TO REQUEST

The failure to identify the proposed action is not excused

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California v. Block, 490 F.2d 753 (9th Cir. 1983) (where Forest Service fails to identify proposed action in DEIS, public is precluded from preparing adequate comments and new draft must be prepared and circulated); see also 40 C.F.R. 1502.9(a) (DEIS must conform to requirements for final EIS)

on the stated rationale that the various "preferred" alternatives have common sub-alternative courses of action. The common elements and their impacts are admittedly kept secret, thus precluding the public from meaningfully participating in the process. Nowhere is the public told who decided what prescriptions were made for particular sites, what management prescriptions were made, where those activities will take place, when they will occur, why those prescriptions were selected over alternative courses of action, or how they were selected.

Failure to include such basic information in an EIS violates NEPA, as does the failure to provide such information to the public upon request. The agency's actual proposal must be incorporated by reference into an EIS. 42 U.S.C. 4332(2)(C). The failure to make available to the public upon request information incorporated by reference is a violation of NEPA. S.O.S./Merrell v. Clark, 747 F.2d at 1248 n. 13 (citations omitted).

The DEIS states that the 1989 proposed budget program was used as a base for consideration of all alternatives. DEIS, pg. IV-5.

"Alternative B is the summation of the individual Forests' 1989 proposed budget programs. It is the baseline for this analysis. The management and environmental consequences of implementing any alternative are expressed as differences from Alternative B. [Elements of Alternative B are] confidential, and the specific dollar amounts and activity levels are not public information."

Id. Necessarily, neither are the specific proposed activities

public information, which is what the EIS procedures of NEPA section 102(2)(C) are all about.

Paul Merrell sought through a Freedom of Information Act request -- within the public comment period -- all records relating to Alternative E "which would reveal the action proposed in the DEIS, its alternatives, and their effects, including but not limited to all proposed site-specific actions." Appendices, Vol. I, pp. 374-380. The response from the Forest Service did not include such records. Id., pp. 381-383.

The Forest Service has thus left the public without a clue to the specific proposals for management activities, even for Alternative B. Instead, Mr. Merrell was referred to Forest level personnel to obtain information at the "project or site specific proposal stage." Id., pg. 382.

This referral to the Forest offices is a straightforward violation of California v. Block, 490 F.2d 753 (9th Cir. 1983), where the Forest Service's second Roadless Area Review & Evaluation ("RARE II") programmatic EIS was held invalid, in part because site-specific details of the Forest Service's plans and assessments were scattered among Forest level offices. The Ninth Circuit held that the public was not expected to travel from Forest to Forest to obtain site-specific details of the Agency's proposed actions. (The referral was also a straightforward violation of the FOIA, 5 U.S.C. 552(a)(6)(B)(i) (requiring agencies to obtain requested records from field facilities).

4. HERBICIDE USE IS ALREADY PLANNED

Another major problem with referring citizens back to the Forests for site-specific details is that the Forest Service has already told the public in Region VI that it is this EIS that will examine the environmental impacts of applying herbicides. If the deciding official does not have the actual proposal in his office, the entire process is an exercise in futility.

In fact, during the entire time that herbicide use has been banned by court orders, the Forest Service has continued to develop land management plans and to set existing harvest levels based on the assumption that herbicides will be used.

"How will herbicides be used? . . . The original issue [on which the Forest Service sought public input] implied that alternative vegetative management practices would be assessed through the forest planning process. A regional EIS is currently being prepared on this concern. The Forest is assuming, for this proposed Forest Plan, that herbicides are available."

U.S.D.A. Forest Service PNW Region, Appendices to DEIS, Proposed Land & Management Resource Plan -- Siuslaw National Forest, pp. A-17-18 (1986) (emphasis added). See also latter DEIS, pg. II-70 (1986) ("The Pacific Northwest Region is currently preparing a Draft Environmental Impact Statement on Vegetation Management. Until the results of that DEIS are final, the Forest assumes, for purposes of Forest Planning, that herbicides are available. Thus the Forest can use all treatment methods, including fire and manual, mechanical, biological, and chemical"). See also id., pp.

1-15, II-70-71 (projecting harvest declines if herbicides and prescribed burning are restricted), IV-4, IV-19; concurrent plan accompanying latter DEIS, pp. III-9, IV-4, IV-93.

It bears notice that the previous Siuslaw National Forest Timber Management Plan also planned the use of herbicides in setting the allowable harvest. See Siuslaw National Forest Final EIS, Ten-Year Timber Resource Plan, USDA FS-R6-FES(Adm)-78-11, pg. 111 (1979) ("The availability of the use of herbicides and prescribed burning is assumed with each alternative") (previous timber management plan litigated in Merrell v. Block). The Forest Service must be operating either under the old or new plan and, despite court injunction, has continuously planned herbicide use clear down to the unit level without complying with NEPA.

Indeed, the DEIS at issue gives specific acreage figures for various management prescriptions under each alternative both in this DEIS and in the Forest land management plan EIS's. Those acreage figures, the claims for the necessity to use herbicides, and the justification for this entire EIS have absolutely no foundation unless the Forest Service has site-specific plans. Furthermore, these acreage figures in this DEIS are admittedly from the secret 1989 site-specific budget proposals. See DEIS at pg. IV-5.

The Forest Service has already developed site-specific plans. The public can not be expected to participate meaningfully unless the Forest Service reveals what action it proposes to take in what

locations. The Forest Service is capable of such an effort. See e.g., II USDA Forest Service PNW Region Final Environmental Statement -- Vegetation Management with Herbicides (1978) (2-1/2-inch-thick volume displaying every proposed spray site in Region VI, detailing chemicals and application methods proposed for each site). (Note that this expired herbicide EIS is the only one in this region that has ever received judicial approval).

5. SITE-SPECIFIC IMPACTS CAN NOT LAWFULLY BE DEFERRED

For at least two reasons, the Forest Service cannot redeem this DEIS by its promise of preparing site-specific environmental assessments: (a). Preparation of environmental assessments rather than EIS's forecloses other agencies and the public from commenting on the site-specific impacts of the Forest Service proposal; and, (b). Many of the site-specific decisions under the umbrella of this DEIS have already been made without public participation, and the Forest Service Regional Counsel has ruled that citizens can not obtain administrative review of such decisions.

NEPA establishes an elaborate procedure for improving agency decisionmaking through preparation of draft and environmental statements that are submitted to the scrutiny of the public and of other agencies with expertise. Environmental assessments, on the other hand, are prepared to document an agency's decision that no EIS is necessary. E.A.'s are therefore not subject to the requirements of public and agency review and comment. The U.S.

Environmental Protection Agency, for example, would never have an opportunity to advise the Forest Service of potential plans to spray herbicides near a known toxic "hot spot," unless a site-specific DEIS is prepared and circulated. Similarly, citizens are precluded from advising the Forest Service of site-specific problems with spray plans (e.g., a pregnancy in the family, children who play on a potential spray-site, etc.) unless EIS procedures are followed.

a. REGIONAL COUNSEL HAS FORECLOSED ADMINISTRATIVE REVIEW

A recent example should illustrate the point. A Five Rivers resident, Ms. Melyce Connelly, recently received a form letter advising her that the Forest Service planned to conduct a controlled burn up to her property line. (Controlled burning of slash is clearly within the scope of the proposed program.) Ms. Connelly petitioned the District Ranger to prepare an EIS, advising the Forest Service that both she and her children were smoke sensitive and suffered medical problems from smoke inhalation. Ms. Connelly specifically raised the issue of whether such activities could be conducted lawfully before this DEIS was finalized. Her petition was denied, upon your Regional Counsel's advice, on the basis that the decision to conduct the burn had already been made in 1983, and had been documented in an E.A. at that time. Ms. Connelly was told,

"The [administrative] appeal period expired on July 20, 1983. An appeal filed at this time

would be considered untimely."^{6/}

The incident is illustrative because Ms. Connelly was entirely denied an opportunity to comment on the proposal, even though the relevant EIS had yet to be published. Despite the fact that she is an adjacent landowner and resident, she was never advised that the E.A. had been prepared. Her comments were never sought. The Forest Service simply shortcut the entire process by making the decision before the EIS had been prepared.

b. PUBLIC WILL NOT BE ALLOWED TO COMMENT

Ms. Connelly's situation mirrors that faced by countless Northwest rural residents. The Siuslaw -- and the other forests in the region -- have planned their use of herbicides and fire years in advance. Only "[i]f the decision is made [in the Competing Vegetation Management DEIS] to not use herbicides [or fire], an assessment and evaluation will be made to determine the magnitude of any economic and environmental effects of that decision" at the site-specific level. Proposed Land & Resource Management Plan -- Siuslaw National Forest, pg. III-9 (1986) (emphasis added).

In other words, the decisions to use herbicides and fire have already been made in the Land Management Plan process, despite the fact that such decisions' environmental effects were never

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The administrative record of Ms. Connelly's petition appears in Appendices Vol. I, pp. 7-13.

considered in that process. Moreover, the Regional Counsel has already ruled that it is too late for citizens to comment on site-specific plans already made. Only environmental assessments -- not subject to EIS circulation and commenting requirements -- will be prepared on any subsequent site-specific plans.

Because the Forests were committed to the use of herbicides long before this DEIS was promulgated, the process used in this EIS is precisely the reverse of that required by NEPA: The decisions to take action have already been made at the site-specific level without NEPA compliance. This DEIS does not address site-specific proposals and impacts; indeed, not even a regional program exists to be assessed in the DEIS. Furthermore, the Forests have announced that the existing land management plans will not be reexamined unless both chemical use and prescribed burning are totally restricted. See e.g., U.S.D.A. Forest Service DEIS on Proposed Land & Resource Management Plan -- Siuslaw National Forest, pp. II-70-71 (1986); see also Siuslaw 1986 Land & Resource Management Plan itself at pg. IV-93 (instructing Forest silviculturists to "[b]ase silvicultural evaluations on an analysis of control methods that are biologically and operationally feasible, such as herbicides") (emphasis added).

In short, the Forest Service has created a gap between two "complementary" EIS's wide enough for the entire "program" to escape site-specific review and comment. This is not good-faith compliance with court orders or with NEPA and the NFMA.

The failure to reveal the site specifics of the program is all the more egregious because the Forest Service -- as demonstrated by the Land Management Plan EIS's and by the decision in Ms. Connelly's case -- does not intend to allow citizens the right to participate even in an environmental assessment process, let alone the site-specific EIS process required by NEPA.^{6/}

C. FAILURE TO CONDUCT REQUIRED RESEARCH

The Forest Service can not thus evade its duty to comply with NEPA. Paul Merrell has already won a judgment from the Ninth U.S. Circuit Court of Appeals instructing the Siuslaw National Forest that it must acquire results of basic research in order to "satisfy its obligations under NEPA to provide an adequate analysis of the effects of the spraying program in the targeted area." S.O.S./Merrell, 747 F.2d at 1247-48. "[T]he agency shall include the information in the environmental impact statement." Id. (emphasis in original).

The issues litigated to a conclusion in that case are final between Paul Merrell and the Forest Service. They are res^{7/} judicata, and are not subject to relitigation. Furthermore,

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See California v. Block, 690 F.2d at 770-72 ("NEPA's public comment procedures are at the heart of the NEPA review process"); id. at 762-63 (EIS must be site-specific); id. at 772-74 (EIS must respond to site-specific comments).

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U.S. v. Stauffer Chemical Co., ___ U.S. ___, 104 S.Ct. 575 (1984). (continued on next page.)

even if such issues were subject to relitigation, the Forest Service still violates NEPA by its failure to obtain and disclose basic research sufficient to make a reasoned choice among alternatives.

1. FOREST SERVICE MUST DO RESEARCH ORDERED BY NINTH CIRCUIT

The DEIS does not even mention the litigation between Paul Merrell and the Forest Service concluded in S.O.S./Merrell, 747 F.2d 1240, reaffirmed by the President's Council on Environmental Quality,^{8/} and implicitly reaffirmed by Congress' 1986 rejection

"The general principle announced in numerous cases is that a right, question or fact distinctly put in issue and directly determined by a court of competent jurisdiction, as a ground of recovery, cannot be disputed in a subsequent suit between the same parties or their privies; and even if the second suit is for a different cause of action, the right, question or fact once so determined must, as between the same parties or their privies, be taken as conclusively established, so long as the judgment in the first suit remains unmodified"

U.S. v. Moser, 266 U.S. 236, ___, 45 S.Ct. 66, 67 (1924); U.S. v. Mendoza, ___ U.S. ___, ___, 78 L.Ed 2d 379, 383 (1984) (reaffirming Moser).

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The Forest Service is also collaterally estopped on the issue of required research because of CEQ's reaffirmance of the research portion of the S.O.S./Merrell decision. Following the decision, the Forest Service and BLM sought its reversal before CEQ. CEQ proposed regulations intended expressly to overrule the Ninth Circuit's interpretation of 40 C.F.R. 1502.22. 50 Fed.Reg. 32234 (August 9, 1985). The draft regulations entirely removed the research duty imposed by section 1502.22(a). Id. After reviewing Mr. Merrell's excerpts of record that were before the Ninth Circuit, CEQ reinstated the original language of subsection (a) with only cosmetic changes, making it clear that the minor changes in the final rule were made only to introduce consistency

of the Forest Service's proposed amendments to FIFRA that would have granted relief from that decision. Instead, the DEIS simply makes conclusory findings that conducting basic research to determine human health effects of herbicides would be infeasible and unaffordable.

The Forest Service already lost the research issue in the Ninth Circuit, however, and that Court did not grant the Forest Service a second opportunity to dispute the feasibility or expense of performing basic research. On the contrary, the Ninth Circuit ruled unambiguously that "The Forest Service Must Do Research If No Adequate Data Exists." 747 F.2d at 1248 (emphasis and capitalization in original.)

"The Forest Service presents no evidence and makes no argument that the costs are exorbitant or that research is impossible. Rather it argues that it cannot be forced to do it. . . . The Forest Service does not, and indeed cannot, cite any case which holds that an agency is not obliged to do research to comply with NEPA. The Forest Service cannot abdicate its responsibility by relying on another agency. It must evaluate the impact of its own actions."

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Id. at 1249.

with the regulation's original title and for clarity. See Final Amendment to 40 C.F.R. 1502.22, ____ Fed.Reg. ____ (Advance Notice, April 22, 1986 at pg. 14) ("the Council has chosen to retain the original format of the regulation"). Mr. Merrell's record before CEQ is included in the Appendices, Vols. III, IV, and V.

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The Forest Service's attorneys, representing BLM co-defendants, also made it clear before the U.S. Supreme Court that they realize the S.O.S./Merrell decision required BLM and the

The Forest Service's proposed findings on the research issue do not even address the Ninth Circuit's ruling, let alone lay out any permissible grounds for seeking relief from the prior judgment, such as mistake, inadvertence, excusable neglect, newly discovered evidence, fraud, etc. See Fed. R. Civ. P. 60(b).

The issue, therefore, is not whether the Forest Service must obtain basic research for this DEIS; nor is the issue what research must be obtained and how it must be treated.

What basic research must be performed as a result of the S.O.S./Merrell decision can not be disputed. The Forest Service recognized in its briefs before the Ninth Circuit that the court order sought would require the Forest Service

"to conduct over ten different tests on each of the herbicides being used[,] [including] oncogenicity studies, reproductive effect studies, metabolism studies, acute oral

Forest Service to conduct basic research:

"BLM must now conduct its own independent research on the health effects, particularly carcinogenic effects, of herbicides. This implicit requirement in the decision below was made explicit in the court's subsequent decision in [S.O.S./Merrell]. BLM has estimated that such research would take at least five years to produce any meaningful result and would cost between \$5 and \$7 million annually. Even if BLM possessed the statutory and budgetary authority to conduct such research, it would still be forced to forego using herbicides on the O&C Act lands for a minimum of five years[,] reducing annual timber yields on O&C Act lands by 25%.

Southern Oregon Citizens Against Toxic Sprays v. Clark, No. 84-267, Government's Petition for Certiorari, pg. 20-21 (U.S. S.Ct. August, 1984); cert. den., 469 U.S. 1028 (1984).

toxicity studies, acute dermal toxicity studies, dermal absorption studies, mutagenicity studies, and epidemiological studies."

Merrell v. Block, Civil Nos. 83-3887, 83-3916, Combined Opening and Answer Brief of the Federal Appellants and Cross-Appellees, pg. 6 (9th Cir.).

2. DEIS MUST DISCLOSE OREGON HUMAN HEALTH STUDY

The DEIS nowhere even mentions the human epidemiological study that the Forest Service was ordered by the Ninth Circuit to include in EIS. Having participated in the conduct of this experiment on human beings, the Forest Service must disclose its results in the DEIS. S.O.S./Merrell, 747 F.2d at 1249 n. 14 ("We find that [the information] would be potentially significant").

The background leading up to this study is informative. On March 1, 1979, the U.S. Environmental Protection Agency suspended forestry and rights-of-way registrations of TCDD-contaminated herbicides 2,4,5-T and Silvex because of animal studies showing multigenerational reproductive effects at the lowest doses of TCDD ever tested, one trillionth of the test animals' body weight administered daily, and because of a human epidemiological study showing a statistical correlation between herbicide use and involuntary human abortions in a 1,600-square-mile area of western Oregon, including the Siuslaw National Forest. (The "Alsea

Study.") ^{10/}

The Forest Service and BLM immediately switched to other herbicides. Soon after Forest Service spraying in Five Rivers in the spring of 1979, valley residents including Ms. Van Strum informed numerous government agencies, including the Forest Service, that "the health of the population of our valley has undergone profound and disturbing change." An epidemic of health problems had struck, including serious respiratory and gastrointestinal problems, vaginal hemorrhaging, and meningitis. Additionally, three of the five pregnant women in the valley (all of those in the sensitive first trimester) miscarried.

Federal Defendants admitted in Merrell v. Block that:

"* * * by mid-August, 1979, a health study was commenced in an attempt to ascertain if there was an association between residence in the Five Rivers Valley and exposure to certain pesticides * * * (O)rganizations who worked on or supplied information in this health study included the U.S.D.A. Forest Service, the U.S.E.P.A., the Oregon State Department of Health, the Lincoln County Oregon Health Department, and the Colorado Epidemiologic Pesticide Study Center(,) * * * operating under a cooperative agreement with the U.S.E.P.A."

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The following history in this section is excerpted from Mr. Merrell's factually undisputed motion for summary judgment in Merrell v. Block. The quotations are from government documents and from written admissions made by the Forest Service and other government agencies in that litigation. The motion for summary judgment appears in the Appendices, Vol. IV, pp. 34-73. A more in-depth treatment of the Five Rivers Health Study, referenced to the record in Merrell v. Block, appears in Chapter 13 of Appendices Vol. 5, C. Van Strum, A Bitter Fog: Herbicides & Human Rights (1983).

During the discovery phase of Merrell v. Block, the Forest Service produced a copy of a letter from the Lincoln County Health Officer, attached to a request from Lincoln County Commissioners requesting that the Forest Service temporarily halt its spray program, stating:

"Preliminary review indicates there is evidence based upon the health histories obtained (that) there was a definite increase in respiratory and gastro-intestinal problems, as well as an increase in vaginal bleeding, following the spraying of 2,4-D in the latter part of May."

The Forest Service denied the County's request for a delay in the spray program, and continued spraying. Five Rivers residents were not to learn until 1981 that they, along with the other residents of western Oregon, had unwittingly been selected to become human guinea pigs in an unlawful multi-agency experiment to determine the human health effects of herbicides used by the Forest Service.^{11/} By then, there had been no live human births

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Such experiments are a criminal violation of the Federal Insecticide, Fungicide & Rodenticide Act, 7 U.S.C. 136j(a)(2)(P):

(2) It shall be unlawful for any person--
* * *

(P) to use any pesticide in tests on human beings unless such human beings (i) are fully informed of the nature and purposes of the test and of any physical and mental health consequences which are reasonably foreseeable therefrom, and (ii) freely volunteer to participate in the test.

See also 7 U.S.C. 1361(b)(2) (making violation a crime and establishing penalties for "knowing" violation). The government admissions and records summarized in the following text,

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in the valley for two years.

Federal Defendants admitted in Merrell v. Block that the Colorado Epidemiologic Pesticide Study Center:

"planned to continue work with these agencies [including the Forest Service] by assisting in the administration of the second health questionnaire in November, 1979 after the end of the current spray season and would specifically be looking for the occurrence of pregnancies and spontaneous abortions, as well as the onset of health effects as may develop.

"Defendants admit that the Colorado Epidemiologic Pesticide Study Center obtained raw data from Lincoln County Health Department questionnaires and performed statistical analyses of that data . . . Defendants admit that the goal of these analyses was to attempt to determine if there was an association between residence in the Five Rivers Valley and exposure to pesticides sprayed in the area.

"Defendants admit that (a genuine document written by defendant Edwin L. Johnson) does refer to the Five Rivers study as being an ongoing study."

Forest Service officials were clearly aware that they were participating in this experiment and were also aware that their role was to administer the poisons for which western Oregon

particularly the Forest Service's 1979 press release, establish that Forest Service and other government officials knew they were participating in a test using western Oregon residents as test subjects.

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Although not made a part of the record in Merrell v. Block, a further relevant case was added in January of 1981 when Zachariah Merrell, son of Paul Merrell and Carol Van Strum, nearly died of a sudden collapse from double streptococcal pneumonia, severe mastoiditis, and a strain of spinal meningitis that a Corvallis medical specialist referred to as "Five Rivers Fever."

residents, including Five Rivers residents, would be the unwitting test subjects. A Siuslaw National Forest press release was provided under the Freedom of Information Act in 1981, stating in part:

"The Siuslaw National Forest is cooperating with the Institute of Rural and Environmental Health, a Colorado group under contract with the Environmental Protection Agency, in their request to review and monitor the fall application program. [Forest Supervisor] Fellows invited [the Lincoln County Health Officer] to participate with the study group in monitoring the program."

An EPA document from the fall of 1979, titled "Status Report -- Five Rivers, Oregon," demonstrated that the study had been expanded to include the entire 1,600-square-mile area of the previous Alsea Study:

"Contact has been made with all hospitals in the Alsea II Study and their cooperation has been obtained regarding abstracting and submitting information on hospitalized spontaneous abortions for the year 1978. The State Health Department has supplied a computer tape of all live births in the state for 1978. The computer tape contains the same data as the previous computer tape for 1972-1977. This latter information is presently undergoing programming to obtain the desired data for the areas previously studied. A post-card survey of the physicians serving the study and control areas is being prepared for administration by the Oregon State Health Department to obtain an estimate of the number of spontaneous abortions which the physicians see and treat in their offices as compared with the number of hospitalized spontaneous abortions."

EPA defendants also admitted that as part of the Five Rivers Study:

"Animal and (human) conception samples submitted for pathological and residue analyses included a deformed kitten [born with four eyes on Carol Van Strum's farm], four ducklings, one chick, and one (human) product of conception [miscarriage]. * * * The product of conception from a confirmed (human) pregnancy and additional samples from a severely deformed (human) fetus born near the Five Rivers area are also awaiting residue analysis in Bay St. Louis, Mississippi." ^{13/}

After the District Court decision in Merrell v. Block, an EPA contract chemist inadvertently disclosed a portion of the chemical analyses involved, showing high levels of TCDD in the sample taken from the baby born without a brain, and in other samples. Following an internal investigation, EPA prepared a report admitting that the TCDD results had been in EPA's possession since 1980 and had been withheld in discovery in Merrell v. Block and in response to discovery requests.

The Ninth Circuit made it clear that basic research on the

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The document's identification of one sample as being from a human fetus later turned out to be erroneous. It was actually tissue from the full-term child of Larry and Laura Archer, Waldport, Oregon, who was born without a brain. A lengthy interview with Mr. Archer is reprinted in Appendices Vol. V, pp. 210-14. C. Van Strum, A Bitter Fog: Herbicides & Human Rights, pp. 210-14 (1983). The same chapter also relates the Archer experience to the unusually high incidence of such birth defects in the same county, which had prompted U.S. Centers for Disease Control to conduct a pilot study to determine if the disorders could be linked to 2,4-D exposure. Completion of the CDC study was lobbied down by forestry interests. Appendices, Vol. I, pg. 158. Raw data from the pilot study appears at id., pg. 159, and articles from The Medical Tribune describing the background to this controversy appear in the immediately following pages, attached to a petition by Lane County, Oregon, commissioners requesting a Congressional investigation of the situation. Such factors should have been addressed in the DEIS.

human health problems in Five Rivers Valley was included in the research it ordered to be performed.

"Soon after [Forest Service] spraying commenced in 1979, numerous and serious health problems were reported in the Five Rivers Valley, including spontaneous abortions, birth defects in humans and animals, and various other illnesses. The EPA began an investigation into these problems, but the Forest Service declined requests by the county health department and board of commissioners to delay the spraying. The Forest Service conducted no research of its own into these problems, and, in its 1981 EA, concluded that the continued use of the herbicides would have no significant impact on the human environment and declined to prepare an EIS."

S.O.S./Merrell, 747 F.2d at 1243. This failure to await research results was held improper. If the Forest Service can not obtain the results from EPA, it must conduct the basic research itself, including both the human epidemiological study and its associated herbicide residue studies. Id., 747 F.2d at 1247. Compare also Id., 747 F.2d at 1243 n. 3 ("we grant plaintiffs the relief they seek in the matters to which the documents relate") (emphasis added) with documents referred to in Plaintiffs/Respondents November 28, 1983 Motion to Supplement the Record, Merrell v. Block, Civil Nos. 83-3916, 83-3887 (9th Cir.) (In Appendices Vol. I, pp. 16-45.

3. FOREST SERVICE MUST OBTAIN LABORATORY TESTS

The DEIS violates the S.O.S./Merrell decision by abdicating to others the responsibility of evaluating the integrity of most

health and safety tests the DEIS relies upon in forming its conclusions. Furthermore, the DEIS and its Appendices repeatedly acknowledge data gaps in the ten crucial areas the Ninth Circuit required to be filled.

a. DEIS DOES NOT ASSESS QUALITY OF DATA

A central issue in Merrell v. Block was whether it was permissible for the Forest Service to merely cite data in the possession of EPA and chemical companies. The Ninth Circuit noted that there is "widespread fraud" in EPA and chemical company data, and that "EPA's data is partial at best, and suspect at worst, because of the testing scandals." S.O.S./Merrell, 747 F.2d 1247 n. 12, 1248 n. 13.^{14/} The Ninth Circuit held:

"The Forest Service could appropriately consider EPA's data on the herbicides in the specific context of the area in which it proposes to spray; it could require the chemical companies, such as amicus Monsanto, to provide the data and necessary research on their herbicides for similar analysis; it could commission studies or undertake its own research or any combination of these options that would satisfy its obligations under NEPA to provide an adequate analysis of the effects of the spraying program in the targeted area."

Id., 747 F.2d 1247-48.

The Forest Service's reliance in its previous Background Statements on EPA and chemical industry summaries of data was

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A more in-depth discussion of the fraud issue, referenced to the record in Merrell v. Block, is included in Appendices Vol. 5, Chapter 12. (A Bitter Fog.)

rejected by the Ninth Circuit as unlawful. Nonetheless, the Forest Service has again repeated the same procedure. Both the DEIS and its Appendices acknowledge that the overwhelming majority of the studies relied upon were not evaluated by the Forest Service or its contractor. Instead, they are merely cited from secondary sources summarizing data or conclusions, primarily EPA documents such as "Tox One-Liners" and regulatory documents.

This is hardly an appropriate response to notification that the herbicide data base relied upon is polluted with "widespread fraud." Mr. Merrell repeatedly demonstrated with EPA documents and with evidence of the Forest Service's prior knowledge in Merrell v. Block that dozens of laboratories conducting herbicide registration studies had prepared fraudulent studies, destroyed all raw data, or had major discrepancies between raw data and reported conclusions. See e.g., Appendices, Vol. III, pp. 95-124 (EPA summaries of laboratory audits). The same summaries of EPA's laboratory audits were on the desk of EPA's former Office of Pesticide Program administrator the same week the Forest Service and EPA decided to use citizens of Five Rivers as human guinea pigs. Id at 95. Furthermore, Mr. Merrell produced evidence that numerous studies cited in the previous EIS had been found to be invalid as part of EPA's investigation of fraudulent laboratory practices. (Summarized in Mr. Merrell's motion for summary judgment, supra.^{15/}

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See also Appendices, Vol. IV, pg. 70 (quoting memo written

The discovery and FOIA requests for all relevant information are still waiting in the record of the case, should the Forest Service decide to reopen the litigation. The information has still not been provided. See e.g., Merrell v. Block, 809 F.2d 639, 642 (9th Cir. 1986) (trial court dismissed Mr. Merrell's motion to compel production of documents on grounds it was moot because of relief granted on merits).

The Ninth Circuit responded to evidence of "widespread fraud" with far more concern than the Forest Service has shown in the present DEIS. The Forest Service was ordered to obtain the data for any studies it relied upon. This obligation to study the actual data was not placed on the public, it was placed on the Forest Service. Mr. Merrell satisfied his burden of persuading the Ninth Circuit that there was a problem with the data. It is no longer his burden, but instead the Forest Service's, both to ^{16/} obtain the data and to evaluate it.

by head of EPA pesticide program discussing EPA's files of evidence demonstrating prior knowledge of the fraud on the part of the chemical companies). The Forest Service, nonetheless, assumes that the chemical companies are a reliable source of information, by repeatedly citing industry summaries of data.

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The Forest Service can not save this DEIS with its claim that it did not knowingly rely upon any studies EPA has found to be invalid. First, such a practice unlawfully abdicates responsibility to another agency to "ensure the professional integrity, including scientific integrity, of the discussions and analyses in environmental impact statements." 40 C.F.R. 1502.24; see also 40 C.F.R. 1500.1(b) ("information must be of high quality [to form] [alccurate scientific analysis]"). Compare S.O.S./Merrell, 747 F.2d at 1243 n. 3 ("we grant plaintiffs the relief they seek") with Merrell v. Block, Civ. No's. 83-3887, 83-

b. FOREST SERVICE HAS NOT 'FILLED DATA GAPS

As previously discussed, the Forest Service and its lawyers recognized before the Ninth Circuit and before the Supreme Court that the Ninth Circuit's decision required the Forest Service to perform basic research to fill over ten types of data gaps for each chemical, including numerous laboratory studies costing millions of dollars and requiring many years to complete, unless the Agency could obtain the data from others. The DEIS and its Appendices list numerous data gaps in these categories, proving that the Forest Service has not complied with the Ninth Circuit's requirement.

c. DEIS RELIES UPON PROPRIETARY SECRET INFORMATION

The DEIS unlawfully abdicates to EPA the responsibility for identifying and assessing the hazards of "inert" ingredients included in herbicides proposed for use, because EPA regards

3916 (9th Cir.) Response and Reply Brief of Paul E. Merrell and Oregon Coast Residents pp. 15-17 (same analysis as above).

Second, eliminating only the studies that EPA has found to be invalid implies that all studies have been evaluated. In fact, Mr. Merrell overwhelmingly demonstrated that EPA has never evaluated the vast majority of studies submitted to it, despite evidence of "widespread fraud" in that data base and that EPA's definition of a "valid" study presumes that studies are valid until demonstrated otherwise. See Mr. Merrell's motion for summary judgment, Appendices Vol. IV, pg. 65. It is preposterous for the Forest Service to assume that the data base is valid, with a background of "widespread fraud," when EPA has consistently claimed that it will take decades to determine whether currently registered pesticides satisfy current registration testing requirements.

"inerts" as confidential business information. DEIS Appendix D, pg. 28. Except for an unidentified petroleum distillate in Esteron 99 and diesel oil in other unidentified herbicides, the identities of "inert" ingredients are not disclosed. The DEIS and its Appendices nonetheless acknowledge that many pesticide "inerts" are not biologically "inert" at all, but include many hazardous substances, including pesticides banned for public health reasons, heavy metals, asbestos, and the like.^{17/} The documents indicate that, generally, neither EPA nor the chemical companies know what inert ingredients are contained in specific pesticides. Furthermore, as discussed below in these comments' treatment of cumulative impacts, chemicals contained in EPA's lists 3 and 4, which the Forest Service assumes would be safe, include "inerts" that will form dioxin compounds in the environment, demonstrating the danger of merely accepting EPA's information without public review.^{18/}

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See EPA documents cited in DEIS Appendices. Note that many of those chemicals, even those on lists 3 and 4, are chlorinated compounds. These or their breakdown products can reasonably be expected to form polychlorinated dioxins when burned. See Appendices, Vol. I, pp. 252-279 (testimony of Dow witness Dr. Otto Hutzinger in 2,4,5-T cancellation proceedings, testifying that PCDD's are unavoidably formed when either chlorine or chloride are combusted in presence of organic compounds). Also notice that lists 3 and 4 also include tall oils and vanillin, byproducts of pulp mill recovery boilers that would be unavoidably contaminated with dioxins.

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See also Appendices, pp. Vol. I, pp. 386-87, 389, 394, (EPA documents indicating that highly-contaminated 2,3,7,8-TCDD wastes have been added as an "inert" ingredient to at least Vertac Corp.'s 2,4-D product and that other TCDD contamination in 2,4-D

History teaches that chemical after chemical allowed into the environment for decades has later been demonstrated to be unacceptably toxic to humans or wildlife. The Forest Service's own experience with 2,4,5-T use in Region VI demonstrates this fact. An assumption that EPA's evaluation of hazards from "inert" chemicals is no more trustworthy. The Forest Service can no more lawfully introduce secret "inerts" into the environment than it could lawfully introduce unidentified "herbicides." See e.g., Oregon Environmental Council v. Kunzman (Kunzman I), 714 F.2d 901, 905 (9th Cir. 1983) (EIS is invalid if it abdicates to EPA responsibility to identify and assess effects of "attendant byproduct" of insecticide); quoted in S.O.S./Merrell, 747 F.2d at 1248.

No law requires the Forest Service to use herbicides. Neither does any law require the Forest Service to line the pockets of chemical companies who dispose of hazardous wastes by

results from Vertac's use of equipment previously used to manufacture 2,4,5-T); see also id., pp. 396-97 (Dow Chemical Co. documents indicating that it sold its Esteron 99 label to Vertac); id., pp. 400-01 (EPA analytical report indicating presence of 1,3,6,8-TCDD and 2,7-DCDD in 2,4-D samples); id. at 401 (also indicating that EPA had earlier found the TCDD isomer in a 2,4-D process wastewater from EPA Region V, i.e., from Dow's plant in Midland, Michigan).

Obviously, the information the Forest Service has received from EPA is less than candid. Furthermore, we understand that Dow Chemical Co. is manufacturing Garlon herbicide on production equipment previously used to manufacture 2,4,5-T. The Vertac experience therefore raises a credible concern that Dow's Garlon is also contaminated with TCDD.

mixing them with pesticides under the guise of adding "inert" ingredients.

These issues are settled. "Any data relied upon in an EIS must be made available to the public." S.O.S./Merrell, 747 F.2d at 1248 n. 13. Inert ingredients form a major portion of any herbicide application and may cause significant human health effects.^{19/} The Ninth Circuit has already ruled that the Forest Service can not simply rely upon EPA and can "require the chemical companies . . . to provide the data and necessary research on their herbicides." S.O.S./Merrell, 747 F.2d at 1247. Reliance on confidential business information is prohibited. 40 C.F.R. 1502.21. California v. Block, 690 F.2d 753, 765 (9th Cir. 1982).

4. ALTERNATIVELY, FINDINGS ON RESEARCH DUTY ARE ERRONEOUS

Even assuming, however, that the Forest Service might somehow

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The Forest Service's reliance on acute toxicity studies of marketed pesticide mixtures to avoid identifying and assessing the impacts of "inert" ingredients is no defense for several reasons: First, acute toxicity studies have no bearing at all on pesticide mixtures' capacity to cause non-acute effects, such as chronic toxicity, cancer, mutations, teratogenicity, systemic toxicity, etc. Second, the Forest Service ignores the fact of "widespread" fraud in such tests and has not evaluated even the acute toxicity studies. Third, the purported acute toxicity studies the Forest Service relies upon can not be traced to publicly available literature. Fourth, at least some of those studies as demonstrated in the old secondary citations the Forest Service relies upon -- such as the acute toxicity studies on Roundup -- are apparently the same old IBT studies identified as invalid in Merrell v. Block. Fifth, as discussed supra, the Forest Service simply ignores the potential hazard of "inert" ingredients once released in the environment, e.g., combustion in slash burns of chlorine or chloride residues from chlorine-containing "inerts" or their degradation products will produce toxic dioxins.

persuade the courts to reopen the Merrell v. Block litigation, the Agency's findings on the exorbitant expense and infeasibility of performing research are fatally flawed.

a. PERFORMING RESEARCH IS FEASIBLE

The Forest Service does not claim that performing essential research is technologically infeasible, except for the claim that it would require at least two years to complete and would therefore unnecessarily delay the program. This argument is specious for several reasons.

First, the Forest Service has already had nearly five years since the injunction was issued and even more time will elapse before the "program" is implemented. Clearly, research could have been performed within this time. Second, even if the courts were to accept such footdragging, this "program" does not end the same year the program is implemented; indeed, the EIS is intended to remain effective for an indefinite period. Thus, even if information were not available in the first year, research could still be completed before spraying is completed, thus affecting subsequent spraying plans through a supplemental EIS. Third, the Forest Service's reliance on the fact that EPA has already requested "many of the outstanding toxicological studies" from the chemical companies -- while admitting feasibility -- simply abdicates the Forest Service's nondelegable responsibility to ensure that adequate study is undertaken before taking action. (Moreover, the statement is irresponsibly vague: what studies are

underway and when will they be completed? What data gaps will remain?) For the foregoing reasons, the real research issue is expense, not technological infeasibility.

b. RESEARCH IS AFFORDABLE

U.S.D.A. Forest Service can certainly afford to conduct the research to determine whether its "program" will kill or maim human beings. The findings the Forest Service has made on its inability to afford such research demonstrates how tightly this Agency has the blinders on. If a federal agency decides to proceed with an action despite scientific uncertainty about potentially significant effects, the agency must prepare "a worst-case analysis weighing the need for the action against all possible adverse impacts." S.O.S./Merrell, 747 F.2d at 1244 (emphasis added).^{20/} The Forest Service has not documented the need for the action in its findings on the exorbitance and

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See also id. (worst case analysis regulation codifies requirement of "analysis of the costs of proceeding without more and better information). The Forest Service has not complied with this requirement because its DEIS is constructed on the foundation of revised 40 C.F.R. 1502.22. The Ninth Circuit has held, however, that CEQ lacked discretion to amend the regulation and that the old regulation remains in effect. Oregon Natural Resources Coalition v. Marsh, 820 F.1051, 1058 n. 8 (9th Cir., 1987). The Forest Service findings are thus outside the procedure required by law.

Furthermore, even if the Ninth Circuit should reexamine its ruling in Marsh, CEQ nonetheless lacked discretion to amend 40 C.F.R. 1502.22 for the reasons contained in Mr. Merrell's comments on the proposed rule change, in Appendices, Vol. II, III.

infeasibility of performing missing research and furthermore has not prepared an adequate worst-case analysis.

First, the Forest Service inappropriately considers the expense of performing all necessary research as a cost only to Region VI. This is not the only Forest Service Region desiring to use these same herbicides, and the expense is legitimately apportioned to all Forest Service programs using the same chemicals. For example, a similar Forest Service program is under development in California. It is preposterous for the Agency to pretend that all expense would have to be borne by Region VI. Similarly, the Forest Service is not the only Federal agency that uses the same herbicides. See DEIS, pg. I-13 (discussing overlapping vegetation management responsibilities of other agencies). The Departments of Interior and Defense, for example, also have major spray programs either established or under development. As a blatant example, the most detailed analysis of the unaffordability of performing research is in the Labat-Anderson literature review in the DEIS Appendices, pp. D(1)8-11. It states that the literature review was prepared, not only to support this Forest Service program, but also a similar program of the Bureau of Land Management. (Presumably, the BLM was willing to share the expenses of preparing the risk assessment that both agencies have substituted for the research ordered by the Ninth Circuit. They should be equally as willing to share the expense of the research itself.) Furthermore, the same pages of the DEIS

Appendices acknowledge that similar studies are already underway by the chemical industry at EPA's request. It is certainly feasible for government agencies to share in that expense. The costs of proceeding without better information are not an expense solely of Forest Service Region VI.

Second, the very fact that many studies are already in progress demonstrates that the expense of filling remaining data gaps is far less than the total costs for the filling the entire missing data base claimed by the DEIS. In other words, if Dow Chemical Company and other manufacturers are going to fill the major portion of the data base, the Forest Service need consider only the expense of performing research still not planned by others. The Forest Service findings are therefore clearly erroneous and not entitled to any deference. This factor underscores the importance of including more specific details of research underway and its completion schedule.

Third, the fact that performing all necessary research is expensive does not support the same conclusion for performing at least some of the research. Performing research on only a few chemicals, for example, would still be of major benefit in assessing the environmental effects of this "program." Indeed, the DEIS indicates at page III-47 that four herbicides alone have accounted for 84 percent of the herbicides used in the Region in the years immediately preceding the injunction; the remaining dozen each accounted for less than 5 percent of the total use. Providing a complete data base on the four major chemicals would

thus significantly increase the Agency's knowledge. Under another approach, the Forest Service could focus on the most important information. For example, conducting studies on leading indicators such as mutagenicity and reproductive effects (with fairly immediate effects) still provides valuable information, even if information were unaffordable on trailing indicators such as carcinogenicity, which in humans may have a latency period of decades. The findings included in the DEIS do not justify foregoing research entirely simply because performing all needed research is expensive. The Forest Service must consider the "costs of proceeding without more and better information," S.O.S./Merrell, 747 F.2d at 1244, not only the costs of proceeding without complete information. (Emphasis added.)

Fourth, the Forest Service nowhere explains why 16 herbicides are necessary to accomplish vegetation management goals. An examination of individual herbicides' relative effectiveness and benefits would likely reduce the number needed, further reducing the expense of filling the research data base. Four herbicides account for 84 percent of the total recent historical use, as mentioned above; what is the cost-benefit ratio if the other 12 were dropped? Such information can be found nowhere in the EIS. ^{21/}

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Such a truncated analysis of alternatives is unlawful.

"[The Forest Service's argument is defective in emphasizing decisional inputs and criteria over the actual results generated by those inputs and criteria. Although it is worthwhile to consider a broad range of

The Forest Service must weigh the need for the proposed action against possible adverse impacts. S.O.S./Merrell, 747 F.2d at 1244.

Fifth, the Forest Service nowhere considers the claimed offsetting savings, realized from use of herbicides, in its consideration of the expense of performing necessary research. Rather than relying upon the single-dimension cost analysis of Labat-Anderson, the Forest Service should have sought the viewpoints of its own economists, who surely would acknowledge that the Forest Service realizes both costs and benefits from the use of herbicides. See e.g., DEIS Appendix B (Forest Service economic efficiency analysis of DEIS alternatives offsets benefits against costs, but never considers costs or benefits of performing ^{22/} necessary research).

Basic research on herbicide health effects is expensive. The Forest Service, however, also realizes both savings and profits

variables in constructing policy alternatives, the procedure becomes meaningless if the variables are assigned numerical values such that only a limited range of outcomes result."

California v. Block, 690 F.2d at 769.

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This case is wholly unlike the APHIS gypsy moth program involved in Kunzman II, 817 F.2d 484, because APHIS -- unlike the Forest Service -- receives no return on expenditures. APHIS was thus justified in using single-dimension cost analysis and its over-all cost-benefit analysis was implicit in its EIS. Id. at 495. The Forest Service, however, is an income-producing unit of government. Single-dimension costing of research expenses therefore can not produce an implicit analysis of the exorbitant expense of performing necessary research, as in KunzmanII.

from the use of herbicides which can be used to offset costs of associated health and safety testing over the life of the "program." For example, Gary Larsen has estimated that Region VI loses over \$8,000,000 per year by not using herbicides.

Appendices Vol I, pp. 4-6. (His is the only publicly-available figure on income foregone by not implementing the herbicide portion of secret Alternative B, the currently planned programs.)^{23/}

Obviously then, Region VI realizes a corresponding savings and profit of over \$8,000,000 per year if it can use herbicides, and this gain should be available to invest in necessary research, with no additional budget outlay. Such savings should be added for each year of the "program's" projected life, properly discounted to present net value. Similar gains should be recorded by other regions of the Forest Service and by other Federal agencies as well. While a total of such funds available for performing necessary research cannot be calculated without contacting other agencies, the Forest Service should make such an effort if their findings are to have any credibility.

The Forest Service's findings should also have reflected the fact that it has continued to accrue such savings during the

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As the only publicly available figure on the savings from foregoing herbicide use, Mr. Larsen's cursory analysis thus forms the only lawful basis for subsequent judicial review of the Forest Service's findings on the claimed exorbitant expense of resolving scientific uncertainty through basic research. See S.O.S./Merrell, 747 F.2d at 1248 n. 13 ("Any data relied upon in an EIS must be made available to the public").

entire time herbicide use has been enjoined. As discussed earlier, the Forest Service has continued to set its timber harvest levels throughout the current planning period on the assumption that herbicides will be used later in the planning period; thus, the Forest Service's projected \$8,000,000 loss has been deferred and will be transferred into a profit if herbicides are used. The Forest Service has already claimed the "earned harvest factors" attributable to future herbicide use. It therefore follows that the Forest Service has already received sale payments for timber it plans to replace through future herbicide use. The funds thus derived are accrued revenues entirely attributable to the planned reduced investment and higher production from herbicide treatments, and the funds should be available to defray associated research expense without increased budget outlays. These accrued funds for the five-year period since herbicide use was enjoined should approach \$40,000,000.^{24/}

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The figure is inexact because Mr. Larsen's figure of \$8,000,000-plus annual benefits to the budget from using herbicides likely includes some expenses, such as road maintenance and noxious weed control, upon which the Forest Service does not receive a direct financial return. To the extent that such programmed disbursements are a legitimate expense of timber harvest (such as roads maintained to facilitate timber removal), however, they should likewise be regarded as expenditures providing a return on investment that should be available for conducting related health and safety research.

We note, moreover, that Forest Service timber is notoriously underpriced compared to private timber in the same markets. It is thus also feasible for the Forest Service to raise timber prices in order to collect funds for adequate study of the public health effects of management practices used to replace the harvested timber, without affecting the agency's relative price position in

Because BLM has already estimated^{25/} that the research the Ninth Circuit has ordered could be conducted over a period of five years with an annual expenditure of \$5-7 million (\$25-35 million total), it appears that Region VI could obviously afford to pay for all necessary research, without financial assistance from any other source, purely from profits it has already taken on the projected use of herbicides.

Furthermore, the Forest Service has already had the estimated five years necessary to conduct such research during the time herbicide use has been enjoined. The Forest Service has unclean hands^{26/} in suggesting that it has insufficient time to conduct such research. The claim can only be made because of its bad faith in failing to prepare an EIS during the time that herbicide use has continuously been proposed at the site-specific level in violation of NEPA. Such bad faith can not equitably be encouraged by allowing the Agency to proceed with an unlawful program.

the market.

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We note, however, that the DEIS makes no findings at all on the expense of assessing the quality of EPA and chemical company information cited, despite the Ninth Circuit's direct order for the Forest Service to perform such assessments.

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The equitable doctrine of unclean hands bars relief to parties whose standing to raise an issue is based on unlawful behavior, as the Forest Service's is here based in claiming that it had insufficient time to complete necessary research. See e.g., National Organization for the Reform of Marijuana Laws v. U.S., 452 F. Supp. 1226, 8 ELR 20572, 11 ERC 1841 (D. D.C. 1978) (denying relief in NEPA herbicide case to organization whose members' standing is based on unlawful conduct).

The Forest Service's finding that it has insufficient time and only "limited funds available" for the studies necessary to fill data gaps is thus clearly erroneous and not entitled to deference. If the Forest Service uses herbicides, Region VI has at least \$8,000,000 per year to allocate to needed research without any change in its budget or increased timber sale price. Furthermore, the agency has already accrued profits sufficient to have performed all needed research. Offsetting savings and profits reduce costs under any valid cost-benefit analysis, and the Forest Service may lawfully only proceed without performing research if the "costs are exorbitant." S.O.S./Merrell, 747 F.2d 1244.

Finally, the Forest Service findings fail to take account of the fact that use of herbicides is only one alternative means of obtaining management goals. While the Forest Service acknowledges that the Regional "program" translates into site-specific proposals each using only one form of control, all preferred alternatives are expressed as a "mix" of management techniques. The relative costs and benefits of the site-specific alternatives, e.g., herbicides, fire, manual brushing, etc., are nowhere compared with and without the costs of research needed for each alternative method. The DEIS comparison of alternatives thus fails to consider "the costs of proceeding without more and better information." S.O.S./Merrell, 747 F.2d 1244.

Surely the most profitable management unit of our nation's Forest Service -- Region VI -- can not credibly claim in the same

document that it will realize huge profits from planned polluting activities, but that it can not afford to spend the comparatively minor sums necessary to determine the associated health costs that must be borne by the public. The log trucks roll by our house every day. The Forest Service makes money on those logs, and can always raise its timber prices to pay for necessary research, thus reasonably allocating the costs of necessary research to those who benefit economically from use of herbicides.

D. DEIS DOES NOT CONSIDER ALL POSSIBLE SIGNIFICANT EFFECTS

Furthermore, even assuming the courts were to ignore the fact that the research issue is res judicata and were further to accept the Forest Service's findings on the expense and feasibility of performing research, the DEIS analysis of possible significant impacts does not meet the exacting requirements of the S.O.S./Merrell decision because it fails to include a worst-case analysis of potentially catastrophic human health effects that may be caused by the Forest Service "program." "Exorbitance" is a relative concept, and the required balancing of need for the action against possible adverse effects must be accurate on both sides of the equation.

The Forest Service makes a series of best-case assumptions, discussed below, to reach its conclusions that various herbicides pose no substantial threat to human health. The Ninth Circuit's decision in S.O.S./Merrell emphasized, however, that merely calling an analysis of risks a "worst-case analysis" does not

establish compliance with the the worst-case analysis duty. In that case, the BLM had prepared a document titled a "worst-case analysis." The Ninth Circuit held that the BLM analysis

"proceeds from an entirely wrong assumption. The BLM admits that no level of exposure to the herbicide has been proven safe, but assumes in the WCA that 'a point is reached at which it becomes clear that no human health effects will occur.' . . . 'Plainly, the worst result that can occur as a result of proceeding in the face of uncertainty as to whether a herbicide causes cancer is that it does cause cancer.'"

747 F.2d 1245-46. The Ninth Circuit elaborated, making it clear that an adequate worst-case analysis must consider "all possible long-range [significant] effects of these chemicals." Id. at n. 9. (Emphasis added.) "If the BLM is to go ahead with the project in the face of these uncertainties, at least it must do so knowing of the fate to which it may be condemning future generations." Id. (emphasis added). The worst-case analysis must project the "worst possible consequences of a proposed action[,] [including] an analysis of a low probability/catastrophic impact event" as well as including "a spectrum of events of higher probability but less drastic impact." 747 F.2d, pp. 1244-45 (emphasis added).

The use of best-case assumptions in analyzing the potentially catastrophic event is thus a violation of NEPA. The Ninth Circuit's analysis makes it clear that if important information is missing and it is in fact unaffordable or infeasible to resolve the issues through basic research, the Agency must assume that the missing research would demonstrate the worst reasonably possible

results that might be brought to light through that research if completed.

As in S.O.S./Merrell, the agencies have once again concluded, on the basis of a threshold probability analysis, that it is unlikely any significant public health problems will occur. This is unlawful.

"'(T)he agency may not omit the analysis only because it believes that the worst case is unlikely.' . . . The duty of an agency is to analyze the costs and environmental effects of the worst case and its costs and then to provide its assessment of the likelihood of the event occurring."

Id., 747 F.2d at 1246 (emphasis in original). In other words, when an information gap triggers the worst-case analysis duty, the agency's duty is simply to admit reasonable possibilities, to say the words, for example:

Catastrophic impact scenario:

"This chemical has not been adequately tested for cancer-causing potential; if our agency uses it anyway, it is possible that our project may cause human cancers. It is possible that this chemical may be a far more potent cause of cancer than any other substance known. Because it is possible that even one molecule of a cancer-causing substance can initiate cancer and the potency of this chemical is unknown, it is also possible that all humans who come in contact with any amount of this substance may contract cancer. Because our project involves spreading this chemical in the environment, it is possible that, as a result, large numbers of people will be exposed to this chemical and contract cancer. Cancer is a horrible, painful disease that is usually fatal. Long periods of time may pass before exposure and its onset. Causing large numbers of human

cancers in order to produce higher timber yields would be unacceptable both to the public and to the agency. Beyond humanitarian aspects, the resulting public backlash might destroy the agency with adverse publicity, lawsuits for damages, and political repercussions. For purposes of this analysis only, we assume that every person who visits a National Forest for any reason or who lives near one will contract cancer.

Medium Impact Scenario:

"It is also possible that this chemical causes cancer, but that it only has a potency in the range of TCDD or aflatoxin. If this is the case, then higher doses than in the catastrophic-impact scenario would be required to produce the same number of cancers. Because exposure would be lower in our project, fewer -- but still unacceptable -- numbers of human cancers might result. We assume for purposes of analysis only that 5,000 cases of cancer would result.

Low Impact Scenario

"It is also possible that this chemical has a low potency for producing cancer, and its use in our project might cause only a very few cancer cases, in which case the decision-maker and the public might find individual risks to be acceptable in light of the need for the project and possible harm to human health. Under this scenario, we might be able to reduce the incidence of cancer by scaling back the program or adopting strict mitigation measures to minimize human contact. We assume for purposes of analysis only that 5 cases of cancer would result nonetheless.

No Impact Scenario:

"It is also possible that this untested chemical does not cause cancer at all. If that is the case, we need not consider risks of causing cancer at all in deciding whether to proceed with the project, and need only consider the chemical's other possible types of significant effects.

This sample language is obviously somewhat over-simplified and abbreviated, and of course examines only one type of effect on one important organism for one alternative proposal. Similar scenarios are easy to write for other types of effects, organisms, and alternative measures. Effects on wildlife, for example, should obviously be assessed in the same way.

The sample language vividly illustrates by comparison, however, the backward nature of the DEIS's risk assessment. Only after saying the words and demonstrating that it has at least considered a range of reasonable possibilities, can the agency then "provide its assessment of the likelihood of the event occurring." S.O.S./Merrell, 747 F.2d at 1246. In other words, only after laying out the plain-language worst-case scenarios -- for each type of possible significant effect -- can the agency assess their probabilities. Only in this way can the agency document that it has "weigh[ed] the need for the action against all possible adverse impacts." Id. at 1244. Only in this way can the Agency demonstrate that it has actually considered reasonable public concerns and give its reasons why it agrees or disagrees.

The DEIS claim that sufficient information is available to predict the outer limits of herbicide effects is factually bankrupt. One need only compare the paucity of data listed in the Appendices with current minimal testing requirements of regulatory agencies to realize that such claims could only be made by the uninformed or by charlatans. See e.g., 40 C.F.R. 158, et seq. (EPA minimum testing requirements); see also 48 Fed. Reg. 11,833

(1983) (announcing availability of 12 volumes of standards and protocols for conduct of tests).

It was established in Merrell v. Block that EPA does not even hope to bring the pesticide registration data base up to current standards for several decades, that not one pesticide on the market today yet meets those standards, and that the Forest-Use Chemicals are far down EPA's list of priorities in assessing the adequacy of existing data. Despite this fact, and despite the fact of "widespread fraud" in pesticide tests that have been performed, the DEIS still assumes that enough information is available to conclude that there is no significant hazard to the public. (If the DEIS drafters sincerely believe it is impossible for more serious effects to occur than those admitted, they should also explain how they were able to accomplish the task, given that even EPA -- with its developed expertise -- has been unable to do so for a single marketed pesticide.)

In short, the DEIS fails to acknowledge the same scientific uncertainty that triggered the worst-case analysis requirement in Southern Oregon Citizens Against Toxic Sprays v. Clark ("SOCATS"), 720 F.2d 1475 (9th Cir. 1982), and in S.O.S./Merrell, 747 F.2d 1240. A plain reading of the court decisions -- even by laymen -- makes it clear that it was scientific uncertainty about public health effects that required worst-case analysis. Indeed, in each case, the plaintiffs' standing to maintain the lawsuits was entirely predicated on that threat.

1. DEIS USES RISK ASSESSMENT, NOT WORST-CASE ANALYSIS

The WCA included in this DEIS is deficient because, despite its subheadings, it addresses only the low- and no-impact scenarios. The reasonably possible catastrophic- and medium-impact events are never acknowledged, the possible effects on public health. Indeed, the probability analyses of the low- and no-impact events unlawfully proceed from the base assumption that no effects will occur. This is unlawful. SOCATS, 720 F.2d 1475 (9th Cir. 1983) (probability analysis must be worst-case).^{27/}

Over-eager to resume their spraying program, the DEIS drafters have unlawfully fallen back on the techniques of "risk assessment" used by federal regulatory agencies rather than attempting to grapple with the relatively new concepts of "worst-case analysis." Risk assessment is an inappropriate tool for developing worst-case scenarios. Risk assessment is incapable of producing a catastrophic worst-case scenario unless nearly all necessary research has been completed, because it has its roots in probabilities rather than in possibilities.

The mere fact that risk assessment is a tool used by EPA in its pesticide program administered under the Federal Insecticide, Fungicide and Rodenticide Act (FIFRA) makes the tool suspect as a method for NEPA compliance. "The EPA registration process is

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See id. at 1479 ("this case involves a lack of information about the probability of any adverse effect").

inadequate to address environmental concerns under NEPA."

S.O.S./Merrell, 747 F.2d 1248 (emphasis added).

While the risk assessment model can absorb some assumptions, it is grounded in the "null hypothesis" of the scientific method. Risk assessment assumes that chemicals are innocent of causing harm until proved otherwise. Risk assessment has, as its basic assumption, a hypothesis that no data exists unless it is added. In the case of a chemical pollutant, for example, the chemical is presumed by the model to be innocent of harmful effects until sufficient numerical data and assumptions -- toxicity, exposure, and individual sensitivity values, for example -- are added to produce unacceptable risk.

Risk assessment can tolerate as many assumptions as are added, but the fundamental bias of the null hypothesis remains. No effects are acknowledged by the model unless numbers are added to make it happen. Only when research data becomes available or when assumptions are added can risk assessment predict an effect. Risk assessment assumes no effects at bottom. Risk assessment is thus more likely to err on the side of understating risks because data has not been developed or because incorrect assumptions have been made. Risk assessment is understandably popular with polluters, because the ability to delay the conduct of research equally delays exposition of the risk.

Risk assessment is the very mechanism that has allowed more than 50,000 inadequately-tested pesticides to remain on the market despite "widespread fraud." Absent data demonstrating

unacceptable risk, chemicals are presumed to be sufficiently safe to use. Even a cursory reading of the S.O.S./Merrell opinion, however, demonstrates that it was precisely this failure of the EPA method that was rejected by the Ninth Circuit. See id., 747 F.2d at 1248 (rejecting reliance on EPA registration because EPA's process allows registration with "less than complete data," i.e., uses the null hypothesis).

Worst-case analysis, on the other hand, proceeds directly to the effect of concern, assumes it will occur, and then asks for evidence that the predicted effect will not occur. See SOCATS, 720 F.2d 1475 (probability analysis must be worst-case). The base hypothesis to be disproved by the scientific method is precisely opposite that of risk assessment. Worst-case analysis assumes that a chemical is guilty of causing harmful effects until sufficient data are added to the probability analysis to demonstrate that it will not occur. It uses a positive hypothesis, as opposed to the null hypothesis in the risk assessment model. The hypothesis to be disproved in worst-case probability analysis is that a chemical will cause harmful effects, not that it won't. Worst-case probability analysis tends to err on the side of overstating risk and forces the agency to admit that there is scientific uncertainty about effects.

In worst-case analysis, risk is assumed, until data disproves the hypothesis. It therefore establishes strong incentives for proponents of the risk, in this case a polluting agency, to

conduct research that might reduce risk perceived by worst-case analysis, or to select alternative methods that do not involve unquantified risks. The known risks of using chain saws to clear brush, for example, can be compared to the possible cancers, birth defects, mutations, and other effects associated with herbicide use. But if the possibility of such effects from herbicides are denied, the comparison is invalid.

Worst-case analysis requires an agency to acknowledge that animal data is not definitive: e.g., humans may still contract cancer even if animal tests are negative.^{28/} By acknowledging uncertainty, the agency can deal forthrightly with possible^{29/} effects and with public concerns.

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A good example is benzene, a "petroleum distillate" which most scientists now accept as a cause of leukemia in humans, and which is both an ingredient and a breakdown product in several of the herbicides proposed for use by the Forest Service. See e.g., S. Epstein, *The Politics of Cancer* 136-137 (1979):

"Attempts to induce leukaemia and other cancers in experimental animals by chronic administration of benzene have on the whole been unsuccessful until very recently. It has been customary to regard benzene as one of two major exceptions to the rule that all chemicals found to be carcinogenic in humans will also induce cancer in animals (arsenic is the other)."

The Forest Service's "worst-case analysis," while based upon tests conducted with purified laboratory grade herbicides, predictably ignores the toxicity of the breakdown products of marketed herbicide mixes and "inert" ingredients. A forthright worst-case analysis would at least acknowledge the possibility of causing leukemia indirectly through such breakdown products. The DEIS, however, admits that herbicides degrade, but nowhere identifies degradation products or assesses their toxicity.

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See Citizens Against Toxic Sprays v. Bergland, 11 ERC 1557, 1559 n. 1 (D. Oregon 1978) ("the Forest Service should understand

Worst-case analysis is unpopular with polluters because they have no choice but to conduct all feasible and affordable research in order to justify continued pollution. It is often far cheaper to switch to less risky technology, and change is of course uncomfortable. Beyond the costs of such research, polluters feel threatened by possible research results, which may confirm rather than disprove the assumption that adverse effects are occurring, raising the spectre of tort liability.

The procedure -- as applied to chemical pollutants -- is diametrically opposite. It is a fundamental shift in the burdens of persuasion and proof. Under risk assessment, those wishing to avoid the risks of pollution -- victims and regulatory agencies -- bear the burden and expense of coming forward with research to demonstrate a hazard. Under worst-case analysis, on the other hand, the proponents of pollution bear the burden of demonstrating safety. Because those who benefit economically from pollution are usually in a far better position to afford necessary research than potential victims, worst-case analysis effectively allocates research expense to the pollutor as a legitimate cost of polluting technology.

Because their beginning points are precise opposites, worst-

that materially misleading statements and conclusions can be just as fatal to an EIS as omissions and false statements. Also, perhaps the public outcry against the use of these herbicides would actually be reduced if the Forest Service were more candid about adverse information in the EIS").

case analysis and risk assessment can not even in theory produce the same results unless there are no remaining data gaps. Which method the agencies must use is a social policy choice, a question of law. The Forest Service can not lawfully shift to the public the burden of coming forth with data by performing a risk assessment rather than a worst-case analysis. This DEIS violates NEPA by using the risk assessment model. The Forest Service's analysis can not lawfully substitute for worst-case analysis, as demonstrated below.

2. RISK ASSESSMENT VIOLATES NEPA

The assumptions in its risk assessment vividly illustrate the Forest Service's absolute refusal to "consider the worst possible effects and the likelihood of these effects occurring." S.O.S./Merrell, 747 F.2d 1244 (emphasis added). The Forest Service "worst-case analysis" does not disclose potentially significant impacts that may result from using herbicides. Id.; see also Foundation for North American Wild Sheep v. D.O.A., 681 F.2d 1172, 1177-78 (9th Cir. 1982) (EIS required if action "may have a significant effect"). The Forest Service has only reached its risk conclusions through a series of best-case assumptions by failing to conduct a worst-case analysis of potential cumulative impacts. This failure underscores the importance of instead requiring that research be conducted rather than resorting to risk assessment.

a. MASSAGING OF RISK MODEL

The Forest Service's contractor, Labat-Anderson, has clumsily committed what is surely one of the most transparent instances yet of data massaging in order to produce desired "risk" results.

The DEIS at page IV-89 reprints a table of herbicide application rates used in the risk assessment, indicating that the table is drawn from Appendix D of the Labat-Anderson risk assessment. The corresponding table appears in the DEIS Appendices, page D(4)-9. According to the accompanying text, the figures under the "worst case" headings are Labat-Anderson's estimates from which the human dose exposure figures were calculated. These "estimates" are most revealing.

Application Rates Used for
Routine-Realistic and Routine-Worst Case Scenarios
(lb active ingredient/acre)

	Aerial		Backpack		Right-of-Way	
	Realistic	Worst Case	Realistic	Worst Case	Realistic	Worst Case
Amitrole	2.00	4.00	2.00	5.00	2.00	8.00
Asulam	2.40	3.34	1.20	3.34	2.40	5.00
Atrazine	3.75	4.00	3.00	4.00	3.00	8.50
Bromacil	0.00	0.00	4.00	10.00	4.00	10.00
2,4-D	2.50	4.00	2.00	4.00	2.50	4.10
2,4-DP	2.00	2.50	2.00	4.30	2.50	5.00
Dalapon	4.00	10.00	4.00	12.00	4.00	10.00
Dicamba	1.00	4.00	0.50	4.00	1.00	3.60
Diuron	0.00	0.00	4.00	6.00	4.00	16.00
Fosamine	3.00	12.00	3.00	11.50	4.00	10.70
Glyphosate	2.00	5.00	1.50	5.00	2.00	5.00
Hexazinone	2.50	3.00	1.12	3.00	2.50	6.00
Picloram	1.00	5.00	1.00	4.00	1.00	2.00
Simazine	4.00	5.00	2.00	4.60	2.00	4.60
Tebuthiuron	1.00	6.00	1.50	6.00	2.20	4.60
Triclopyr	2.00	8.00	2.00	8.00	2.00	8.00

For example, under the "Aerial" application column, the "realistic" exposure for Atrazine is 3.75 pounds per acre while the "worst case" application rate is 4.00, only a slight increase. For Tebuthiuron, however, the "worst case" exposure rate is six times the "realistic" application rate. No two "worst case" and "realistic" pairs of rates in the entire table have the same proportional differences. Three chemicals, for example, have realistic application rates of one pound per acre, but their corresponding worst-case rates are 4, 5, and 6 pounds. A 3-pound realistic rate for Fosamine produces a four-fold worst-case rate, while a 4-pound Simazine realistic rate only increases by 25 percent to provide a 5-pound worst-case rate. Labat-Anderson gives no basis whatsoever its "estimates" of worst-case application rates.

The selection of the "estimated" application rate -- the root of the "exposure" analysis -- is the key assumption that is used to manipulate the entire risk assessment.^{30/} There is no logical

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Labat-Anderson's reliance on Forest Service monitoring data as reported also erroneously assumes that such data is reliable. The bibliography of the EIS cites a study of the accuracy of results from contract laboratories used by the Forest Service. (Norris, 1985). Inexplicably, we have not been able to find any discussion of this study in the EIS. The study is important because, according to data in Dr. Norris's manuscript version of the study, the clear trend was toward a likelihood of false negative and understated results. The study demonstrates that

basis, however, for assuming that an 'accident involving Tebuthiuron would produce an exposure six times the application rate, but that an accident involving 2,4-DP would only increase exposure by 25 percent. The only possible explanation is manipulation of assumptions to produce desired results rather than an honest "risk" assessment. What Labat-Anderson has apparently done is to adjust the worst-case application rate to reflect the toxicity value, thereby producing the desired risk levels.

Labat-Anderson's technique is well-recognized in the scientific community.

"The inexperienced or naive analyst might perceive the lack or sparcity of data to be a minor handicap. The fact of the matter is that a superfluity of data is extremely confining, imposing severe constraints on the technique, imagination and creativity of the analyst. On the other hand, a lack of data permits full exercise of one's talents and abilities.

The ideal situation is to have absolutely no data available at all. Unfortunately, this situation is seldom realized in practice. Nevertheless, this case is of great theoretical importance, being analogous to frictionless pulleys, massless strings, viscuousless fluids and lossless circuits with

Forest Service monitoring data is unreliable without blind confirmation analyses. Similar concerns cannot be assessed about the unidentified laboratories used to analyze control samples, because samples were not submitted to them blind. See Appendices, Vol I, pg. 83 (Norris manuscript). See also E.D.F. v. Froehlke, 473 F.2d 346 (8th Cir. 1972) (EIS must include results of agency's own investigations). The worst-case analysis therefore should have assumed that actual field exposure data is unreliable, or at the least vastly understated.

which physicists, engineers, and other lower life forms waste their time.' Too, this ideal, referred to as the Null Data Set (NDS), may be approached asymptotically by the skillful analyst . . .

Failing to achieve the NDS, the analyst should endeavor to achieve the Minimal Data Set (MDS). With a single datum, obviously, the slope of the curve and all higher derivatives are unconstrained. With two data, the second and higher derivatives are unspecified, etc. Obviously, a degree of freedom is lost with the addition of each additional datum. This illustrates intuitively the essentiality of employing an MDS. . . . A master analyst can, of course, draw from a plethora of esoteric statistics to support his hypothesis. . . .^{31/}

The point is that if risk assessment has any utility at all, it must be used in an honest quest for knowledge, rather than to whitewash a problem. The entire process of risk assessment is so easily and often manipulated by charlatans posing as scientists that the time to pay it credence in this society may have passed. Assumptions used as substitutes for hard research data possess only the integrity of the persons who make them. The Forest Service clearly erred in proposing this "risk assessment" as an alternative for the hard information required by S.O.S./Merrell.

E. HERBICIDES MAY CAUSE CUMULATIVE IMPACTS

The Forest Service erroneously states that cumulative impacts

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Else, Sum Huan. Intelligence Analysis of Unavailable Data, 28 J. of Irreproducible Results (4):28-29 (1983); in Appendices Vol. III, C. Van Strum comments on proposed changes to 40 C.F.R. 1502.22, pg. 9.

from the herbicide program "would be insignificant." See e.g., DEIS Appendices, pg. D-5(32). To the contrary, there may be significant impacts, and the Forest Service is therefore obligated either to perform research to resolve the uncertainty or to prepare a worst-case analysis on potentially significant cumulative impacts. Oregon Natural Resources Council v. Marsh, 820 F.2d 1051, 1059-60 (9th Cir. 1987) (NEPA requires worst-case analysis of potential cumulative impacts); see also Thomas v. Peterson, 753 F.2d 754, 757-761 (where timber sales are programmed on basis of planned actions, cumulative effects analysis may not be lawfully deferred to site-specific EA's; foreseeable connected projects must be assessed in EIS).

As Labat-Anderson's massaging of exposure data to produce desired results demonstrates, human exposure is the critical assumption that drives the Forest Service's entire risk assessment.^{32/} The Forest Service nowhere assumes, for purposes of analysis, that the public is already exposed to toxic levels of pollution, as they were directly ordered to do both in SOCATS and in S.O.S./Merrell.^{33/}

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Having been told by the Ninth Circuit that it must concede the possibility its chemicals will cause toxic effects, the Forest Service now retreats into hiding the effects of its program by making best-case assumptions about exposure (dosage) on the other side of the hazard equation. This retreat is unlawful because the Forest Service was ordered to assume adverse public health effects in its worst-case scenarios.

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Puzzlingly, the Forest Service worst-case analysis only assumes significant harmful effects on employees and applicators, but not on the public. Assuming the Forest Service did read the

a. THE LAW ON CUMULATIVE IMPACTS

In layman terms, the cumulative impacts assessment duty is illustrated by the adage about the straw that broke the camel's back. That bit of folklore teaches that living beings' ability to tolerate added burdens has definite limits.

Adding to pollution levels already in the environment is entirely analogous. As the DEIS recognizes, toxic effects occur when enough toxic exposure occurs to evoke an effect. If humans have already been exposed to enough of a toxic substance, addition of even a minute quantity may cause a toxic effect to occur, i.e., to break the camel's back.

The DEIS -- while acknowledging that humans are exposed to unknown quantities of chemical pollution as a result of other human activities -- nowhere assumes for purposes of worst-case analysis that the public is already has sufficient toxic exposure that the added toxic stress of this "program" may cause

S.O.S./Merrell, decision, it has fundamentally misunderstood the panel's remarks at 747 F.2d 1246 n. 8. The panel did not say that a worst-case analysis would be adequate if it only addressed effects of accidental massive doses on applicators or of spills directly on employees, as apparently interpreted by the Forest Service. The panel's remarks in that regard were describing the worst-case analysis prepared by the BLM that it was holding inadequate. ("We do not imply that the contents of the WCA prepared by the BLM should not be part of the amended WCA,]" etc. Id. (emphasis in original). In other words, the panel was telling the agencies why the previous worst-case analysis did not go far enough: because it only assumed the possibility of effects on workers and applicators. Compare id. with Appendices, Vol. I pg. 80 (BLM WCA held invalid by Ninth Circuit).

significant toxic effects. This failure is particularly important because of the Forest Service's assumption^{34/} that toxic thresholds do exist for many types of effects.^{35/} If the public

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The DEIS should bluntly state that toxic thresholds are assumed under the risk model, not because the existence of such phenomena has been established, but because mathematical models have not yet been developed for extrapolating from higher doses to lower doses for some kinds of effects, such as reproductive effects. See e.g., Appendices, Vol. I, pg. 51 (EPA TCDD risk assessment document acknowledging that reason for reliance on NOEL concept is that "there are no generally accepted mathematical extrapolation models for estimating reproductive/developmental toxicity below the dose ranges tested in animal studies"); id. at 203 (Ruckelshaus speech). In fact such thresholds may not exist, because reproductive studies are subject to the same statistical limitations as cancer studies, discussed in the Qualitative Risk Assessment, DEIS Appendices pg. H-3. The DEIS use of a NOEL is a best-case assumption that can not possibly be supported by evidence. It is fundamentally misleading. Moreover, the Forest Service already lost the issue on whether it can assume the existence of a threshold for teratogenic effects. See S.O.S., 747 F.2d at 1242, 1244 (decision applied to teratogenic effects; BLM must prepare worst case analysis "bottomed on the assumption that its herbicides are not safe").

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For toxic effects that the Forest Service assumes have no thresholds, failure to assume already toxic body burdens also ignores the fact that relative incidence of effects from the same incremental exposure increases exponentially on the risk models used by the Forest Service. Thus, the same incremental dose produces a higher incidence of cancer at higher cumulative doses.

The use of a NOEL assumption is inappropriate for worst-case analysis purposes absent some evidence that such thresholds actually exist.

"For example, a low dose of chemical with a reproductive effect (e.g., effects on oogenesis, fertility or conception) may result in a slight impairment of fertility in all exposed individuals. With the endpoint of cancer, however, only some of the exposed individuals will contract the disease."

U.S. National Research Council & Royal Society of Canada, The Great Lakes Water Quality Agreement: An Evolving Instrument for Ecosystem Management, pg. 72 (1985); in Appendices, Vol. VI, C.

has already reached or passed the toxic threshold, the Forest Service's incremental added exposure can produce profound toxic effects.

The Forest Service must also consider whether "the action is related to other actions with individually insignificant but cumulatively significant impacts." 40 C.F.R. 1508.27(b)(7). The DEIS suggests, however, that because treatments in the same areas will be infrequent, incremental exposures will therefore be low and no significant cumulative human health impacts will occur. This ignores the fact that the environmental camel may already have all the toxic load he can bear. "Significance cannot be avoided by terming action temporary or by breaking it down into small component parts." 40 C.F.R. 1508.27(b)(7).

"'Cumulative impact' is the impact on the environment which results from the incremental impact of the action when added to other past, present, and reasonably foreseeable future actions regardless of what agency (Federal or non-Federal) or person undertakes such other actions. Cumulative impacts can result from individually minor but collectively significant actions taking place over a period of time.

40 C.F.R. 1508.7 (emphases added). The Forest Service's analysis must "consider cumulative impacts of the proposed actions which supplement or aggravate the impacts of past, present, and reasonably foreseeable actions." Marsh, 820 F.2d at 1059.

Van Strum & P. Merrell, No Margin of Safety: Dioxin Pollution and the Need for Emergency Action in the Pulp & Paper Industry (1987), pg. IV-13 n. 25.

The Forest Service's worst-case analysis, however, does not admit of any uncertainty due to background levels of toxic chemicals already in the environment, whether caused by the Forest Service, by other government agencies, or by private parties. Indeed, the worst-case analysis does not even consider the cumulative effects of its own "foreseeable" site-specific actions. This is a violation of NEPA. Indeed, the DEIS description of the affected environment is fatally deficient because it never adequately describes its already-polluted nature.

1. WCA MUST ASSUME TOXIC LEVELS OF TCDD IN THE ENVIRONMENT

While admitting that 2,4-D is contaminated with certain dioxins of unknown toxicity and that prescribed fire will produce dioxin pollution,^{36/} the DEIS nowhere assumes for purposes of worst-case analysis that the public is already exposed to toxic levels of dioxin from a variety of sources, both governmental and

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No reference to data is given for either statement. The DEIS should have done so for such crucially important information. We are aware of some information suggesting that 2,4-D is contaminated not only with lesser chlorinated dioxins, but also with TCDD, as discussed above. We have not seen recent studies of dioxin residues from prescribed fire, but assume this is the work of the Forest Service's Dr. Logan Norris, whom we understand has been gathering air samples around the Siuslaw National Forest area, e.g., near the Fall Creek fish hatchery, and who has also been engaged in a cooperative study with Oregon DEQ to study herbicide -- and dioxin -- residues associated with private herbicide spraying and burning operations in the Oregon Coast Range. We are precluded from commenting on the analysis of such data when it is presented only as generalized conclusions without exposition of the underlying data.

private in origin, resulting in a worst-case scenario of unacceptable cumulative impacts from this program.^{37/}

EPA has recently reported that "sizable segments of the general population of the industrialized world may be exposed to about 1 pg/kg/day of 2,3,7,8-TCDD, probably from a variety of sources."^{38/} This equates to a "background" of 2,3,7,8-TCDD-equivalent exposure consisting of one-trillionth of human beings' body weight daily.

Such information raises a legitimate scientific concern that the Forest Service's program may have significant incremental effects because of the incredible toxicity of dioxin compounds and their persistence.

For example, EPA also recently reaffirmed its concerns that a Dow Chemical Company three-generation study with rats may not have demonstrated a "no observed effect level" ("NOEL") even at the lowest dose tested, one trillionth of the test animals' body weight administered daily.^{39/} EPA's application of standard

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This failure is unforgivable. Even regulatory agencies using the risk assessment model now believe that effects of various dioxins and furans are additive. They assess the toxicity of furans and dioxins other than TCDD by expressing their "toxic equivalent factors" (TEF's) in relationship to TCDD. See e.g., Appendices Vol. I, pp. 420-485 (EPA guidelines for assessing relative risks of dibenzo-furans and dioxins other than TCDD.

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Appendices, Vol. I, pp. 47. 52-54 (EPA Draft TCDD cancer risk assessment).

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Appendices Vol. I, pp. 55-20 (EPA reproductive effects assessment from new dioxin risk assessment, relating assessment to situation of forest herbicide spraying in Oregon). Compare latter with EPA's previous position on this study, summarized and

risk assessment safety factors produced an acceptable "reference dose" of 10 quadrillionths of a person's body weight to protect from unacceptable multigenerational reproductive effects.^{40/}

Humans on the average may thus already experience a toxic daily dose of dioxins to which the Forest Service program would add, raising a reasonable possibility of multi-generational reproductive effects in humans.

This concern for possible cumulative impacts on human health is not specious. EPA relied upon the same three-generation rat study to support its 1979 conclusions that use of dioxin-contaminated herbicides may have been responsible for an increased incidence of involuntary human abortions in the Siuslaw National Forest area (the "Alsea Study").^{41/}

(The Forest Service nowhere mentions the fact that it had used these dioxin-contaminated herbicides extensively throughout

referenced in Appendices Vol. VI, Chapters I-III (EPA relied upon study to support conclusion that TCDD was causing unacceptable involuntary human abortions in Five Rivers/Alsea area of Region VI). The EPA reproductive assessment refers to several studies demonstrating that TCDD produces in mammals the type of kidney nephrosis our son Nicholas was born with.

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Appendices, Vol. I, pg. 16. Note, however, that this value was erroneously transcribed into the EPA draft cancer risk assessment as being two orders of magnitude higher, i.e., one trillionth of a person's body weight. See id., pg. 51. This understates calculated risk by a hundred times, which may explain why EPA chose to concentrate on carcinogenic effects instead.

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USEPA. Decision & Emergency Order Suspending Registrations for the Forest, Rights-of-way, and Pasture Uses of 2,4,5-Trichlorophenoxyacetic Acid (2,4,5-T). 44 Fed. Reg. 15874 (March 9, 1979).

the "program" area for more than 30 years prior to EPA's ban.)^{42/}
 Failure to consider the additional cumulative effect from this past exposure is a violation of NEPA. The Forest Service clearly has such information; it was an active party to the 2,4,5-T cancellation proceedings and received all filings. One can only speculate on why Labat-Anderson did not use it in the worst-case analysis:

"One of the questions most frequently asked by students in this field is 'But what can be done about data supplied by the customer?' One solution is simply to ask the customer to provide less data. Since the collection of data is expensive, and the customer undoubtedly has many other ways to squander taxpayers' money, he may be more than happy to comply. After all, by reducing his collection costs, he can fund more analysis, and more importantly, more reports. Since efficiency is defined as the number of report pages produced per unit cost (units of decipages per kilobuck), this will tend to increase his efficiency rating and thus his budget next year."

Appendices Vol. III, pg. 10 of C. Van Strum comments on proposed amendments to 40 C.F.R. 1502.22.^{43/}

Some data, however, are too well known to be so easily ignored. For example, EPA's existing current water quality guidelines for guidance to states and other agencies forecasts an

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See Appendices Vol. I, pg. 12; C. Van Strum, A Bitter Fog: Herbicides & Human Rights, pg. 12 (first experimental forestry use of phenoxy herbicides nationally occurred near Five Rivers in 1948).

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Else, Sum Huan, Intelligence Analysis of Unavailable Data, 28 J. of Irreproducible Results 28, 29 (1983).

unacceptable "one-in-a-million" incidence^{44/} of cancer from human exposure to TCDD in the low parts-per-quintillion range.^{45/}

The available evidence suggests that hazardous levels of TCDD

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Because NEPA requires EIS's to be written in plain language readily understood by laymen, the Forest Service should limit its use of the word "risk" and substitute "incidence" wherever appropriate. "Risk" is a term of art that in our experience most people misunderstand as a game of chance. When toxicologists discuss "risk," they are discussing risk to the individual as reflected by estimated incidence in the population. Laymen who hear phrases such as, "the risks of cancer are three-in-a-million," tend to understand the phraseology to mean that the risks of producing any cancers at all are only three-in-a-million, when the information actually sought to be conveyed is that out of every million persons exposed, three may contract cancer. In other words, the public and the decisionmaker might believe that the onset of cancer is a game of chance, rather than a predicted effect of the program. Substitution of such terminology is thus critical, because hopefully the public and the decisionmaker might be unwilling to accept that \$8,000,000 worth of annual benefits to the Forest Service are worth causing cancer even in a single fellow human being.

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Appendices Vol. II, USEPA Ambient Water Quality Criteria for 2,3,7,8-Tetrachlorodibenzo-p-dioxin. (1984). Note, however, the Criteria's assumption that fish only bioconcentrate TCDD in their tissues to levels 5,000 times background levels. It bears notice that new research from U.S. Fish & Wildlife Service has demonstrated a bioconcentration factor in trout between 39,000 and 86,000 times background levels. (Also included in Appendices Vol. II.) Substituting the research results for EPA's assumption in its water quality criteria risk model produces -- by our calculations -- an unacceptable incidence of cancer from water polluted in the mid-parts-per-sextillion range. The Forest Service's worst-case analysis must reflect such new information.

The Forest Service should also have addressed the fact that in the same study, the Fish & Wildlife Service researchers could not find a no-effect level on rainbow trout hatchlings clear down to the lowest contamination level tested, 38 parts-per-quadrillion in water. Because of the trout's biological similarity to commercially important members of the salmonidae family, the DEIS should also have projected significant declines in salmon runs from additional toxicity added by the burning and spraying program.

from previous Forest Service spray projects still linger in the proposed spray areas, raising further concerns that there may be significant cumulative human health impacts.

For example, EPA returned to Five Rivers in 1984 and found levels of TCDD in the sediments of a feeder stream approximately four times the levels that were found at the same site in 1979, before the 2,4,5-T ban.^{46/} EPA stated in its report that the levels in fish would roughly correspond to the levels in the sediments, which were in a range in the various samples up to 78 parts-per-trillion of TCDD. EPA specifically stated that it believed the most likely explanations for the levels found were from 2,4,5-T spraying before the ban or from prescribed burning and that it believed levels in fish would correspond to the levels in the sediments at a 1:1 ratio.^{47/}

Likewise, EPA's recently-completed National Dioxin Study indicated that a former Forest Service 2,4,5-T helicopter loading site not used since the late 1960's was still contaminated at up to 6.623 parts per billion of TCDD.^{48/} Similar sites are

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Appendices Vol. I, pp. 212-224. Levels from 1979 at the same site were reported by Lane County Commissioners as 17-20 parts-per-trillion. Id., pg. 114.

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Appendices, Vol. I, pp. 73-75 (EPA National Dioxin Study report (noting that USDA Forest Service "has restricted access to the contaminated heli-pads," and has "indicated willingness to sponsor a pilot project" for site detoxification). Note also results only one order of magnitude lower in Mark Twain National Forest, Missouri, and Santiam Forest, Oregon. Id. These results

scattered throughout Region VI, a toxic legacy of the Forest Service's previous addiction to 2,4,5-T until it was banned. ^{49/}

Attached to the Lane County Commissioner's request for a Congressional investigation are EPA documents showing a variety of other TCDD contaminated samples from western Oregon. Appendices Vol. 1, pg. 114. Fifty to 75 percent of western Oregon deer and elk samples were contaminated with TCDD at levels up to 68 parts per trillion. An EPA document indicated the reason for the latter study was the Oregon deer and elk had "an excess" of reproductive

provide best-case boundaries. At rock bottom, the DEIS should have identified all such helipads in Region VI, assumed at least equivalent contamination, and assessed additive and synergistic risks to downstream residents. The Forest Service should also consider conducting throughout Region VI the same type of mitigation it is apparently willing to do in Arizona. ^{49/}

Ironically, the Forest Service concluded in a series of "worst-case" analyses in its 1978 Region VI herbicide EIS, pp. G-10-11, that TCDD-contaminated 2,4,5-T would have no human reproductive effects using an analysis similar to the "worst-case" analysis included in the present DEIS. Rudimentary comparison of the mitigation measures in that EIS with those in the present DEIS demonstrate that the Forest Service's mitigation measures for 2,4,5-T were even more stringent than those applied to the newer, less-thoroughly tested chemicals. Nonetheless, the Forest Service found out through EPA's ban on 2,4,5-T spraying only a year later that it had apparently caused involuntary human abortions in the Siuslaw National Forest area. The DEIS lawfully should have provided some basis for believing that its mitigation measures are somehow more effective than before, rather than merely listing them. Marsh, 820 F.2d at 1055 (mere listing of mitigation measures insufficient; EIS must analyze mitigation measures in detail and explain effectiveness). We also note, as discussed earlier, that the DEIS mitigation measures have not been made mandatory or subject to regional review for compliance, even assuming their effectiveness had been evaluated. Lacking enforceability, they are meaningless.

problems, including stillbirths and birth deformities. A child born without a brain (the Archer baby) had 3 parts-per-trillion TCDD in its tissue. Oregon mothers' milk samples were almost uniformly contaminated with TCDD. A document requesting further funding for dioxin analyses including Oregon analyses, signed by the head of EPA's pesticide program, stated that "dioxin monitoring requirements and responsibilities are increasing because dioxin contamination of pesticides and the environment are also increasing." A table of analytical results (Table VII) that was presented to the Ninth Circuit in S.O.S./Merrell showed up to 5800 parts-per-trillion TCDD in Alsea area sediments and sludges.

Region VI's 1978 Vegetation Management with Herbicides EIS also acknowledged, under court order, that earlier signs pointed toward widespread contamination of the human environment with TCDD. In its appendices, it included positive TCDD results of mothers' milk analyses from the Siuslaw National Forest area. Data showing contamination of beef fat was displayed. Another table showed small wildlife samples contaminated up to 143 parts-per-trillion.

TCDD human exposure also occurs through paper products contaminated up to 50 parts-per-trillion in the manufacturing process. Appendices, Vol. I, pg. 281. Acceptable risk levels are exceeded, even at 10 parts-per-trillion, for persons who drink coffee made with bleached coffee filters. Id., pg. 285 (EPA risk assessment, but note that EPA did not assess risk from cumulative exposure to all contaminated paper products, including tampons,

sanitary napkins, toilet and facial tissue, food container board and wrapping, cigarette papers, disposable diapers, and geriatric accessories).

Pulp and paper mills also produce dibenzo-furans, TCDD, and other dioxins, at hazardous levels both in effluent and emissions. Appendices, Vol. VI. The map on the following page shows the locations in Region VI where EPA and the pulp and paper industry are currently studying TCDD pollution levels, both in plant discharges and in fish.^{50/} The affected airsheds should reasonably be assumed to be polluted with dioxins.

EPA recently released documents indicating that picloram, one of the herbicides proposed for use, is contaminated with high levels of hexachlorobenzene, and also indicating that hexachlorobenzene is "known to be contaminated with polychlorinated dibenzodioxins and dibenzofurans." Appendices, Vol. I, pp. 406-07, 409. EPA said:

"HCB poses human health and environmental risks very similar to those posed by other fat-soluble toxicants such as DDT and PCB's. However, studies during the past 10 years have indicated that, while the concentrations of DDT and PCB's in human adipose tissue in the U.S. have been decreasing, the level of HCB has remained the same or, possibly, increased. In an EPA human tissue monitoring study, ninety-eight percent of human fat samples had HCB at measurable levels (50 ppb to 100 ppb)."

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We prepared the map with information drawn from EPA documents such as the table included in Appendices Vol. I, pg. 280, which we recently received from EPA under the FOIA.



Locations of 20 Pacific Northwest pulp mills using chlorine, currently being studied by EPA to determine the extent of their TCDD pollution. From Appendices, Vol. I, pg. 280.

Id. at 405. The DEIS worst-case analysis nowhere examines the cumulative impact of past and planned use of picloram contaminated both with HCB and dioxins and furans. This is particularly important because "[a]vailable evidence indicates that HCB is very resistant to chemical and biochemical degradation." Id.

Likewise, the same EPA document indicates there may be a cumulative dioxin and furan hazard from any pesticide mixture including chlorine as an ingredient at any stage of manufacture. This is logically inferred from the information already quoted and from EPA's further statement that HCB is inevitably produced from the manufacture of chlorine. Id. at 412. If dioxins and furans are inevitable contaminants of HCB, and HCB is likewise an inevitable contaminant of chlorine, it thus follows that dioxins and furans are inevitable contaminants of chlorine manufacture.

This information stresses the importance of assuming that all herbicides proposed for use and used previously contain dioxins, without complete and publicly accessible information about herbicide ingredients and contaminants. ^{51/} It also stresses the

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Notice particularly, paper presented at last annual international dioxin symposium indicating that the higher-chlorinated octa-dioxin breaks down in soils to 2,3,7,8-TCDD, which is a "sink." Appendices Vol. I, pg. 367 (notes of Dr. Donald Barnes, Chairman of EPA's secretive Chlorinated Dioxins Working Group, the Agency's coordinating body for dioxin and furan research and regulatory action). This discovery counsels a far higher Toxic Equivalent Factor for octa-dioxin than used presently by EPA; it also demonstrates the dangerous fallacy of relying on the risk assessment model. Dr. Barnes' notes are a useful index to the many important papers presented at the symposium, which will be published in a special edition of Chemosphere later this year. The cumulative number of dioxin sources revealed at the

importance of making similar assumptions about private spraying in the affected area, which the DEIS Appendices admits has occurred.

There is no scientific basis for assuming that the public will not be exposed to cumulatively toxic levels of herbicides, smoke, contaminants, "inerts," and the residues of previous polluting activity. The "No Observed Effect Levels" have already been exceeded for dioxins. There is also no scientific basis for concluding that dioxin will not have additive or synergistic effects with proposed spray or burn operations. As the head of EPA's Carcinogen Assessment Group testified in the 2,4,5-T cancellation hearings,

"The human population is exposed to a large number of carcinogens in the environment. Therefore, it is possible that exposure to a potent promoter such as TCDD could increase the number of cancers induced by environmental carcinogens and shorten the latency period for the development of cancer. . . . There is no theoretical basis for making even ballpark estimates of the risk posed by promoters and cocarcinogens to exposed persons because the mechanism for promotion is not well understood and because the degree of total exposure of the human population to the numerous carcinogens in the environment cannot be well quantified. However, it is possible that TCDD could significantly increase human cancer as a promoter or cocarcinogen at exceedingly low levels of TCDD exposure."

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Appendices Vol. VI, pg. II-7.

conference should be carefully evaluated in a revised or supplemental DEIS, so that the public can comment meaningfully on the Forest Service's analysis.

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The DEIS assumes that acute toxicity studies of pesticide

Major burning and spray programs do not take place in a pollution-free environment, nor do they involve the use of pure laboratory-grade active ingredients, which almost all toxicological studies cited by the Forest Service used. Even EPA's "best-case" risk assessment procedures assume that the effects of dioxins and furans are cumulative. The Forest Service can scarcely claim credibly that it uses EPA's methods while ^{53/} ignoring them.

"[The 'dioxin' we all refer to is but one member of a chemical family. . . . Members of another closely related family of chemicals, the chlorinated dibenzo-furans, are frequently found as contaminants in products that contain dioxins. The pattern of disease that the other toxic dioxins and dibenzofurans produce is indistinguishable from that observed with TCDD. Basic research with these chemicals indicates that a common mechanism is probably involved in their toxicity. Therefore, the public health risk should be assessed by calculating aggregate exposure to all of these chemicals, not only to TCDD."

J. Moore, EPA Assistant Administrator, in testimony, Dioxin--The

mixtures somehow demonstrate that there is no synergistic hazard from active ingredients and their contaminants or "inert" ingredients. There is no scientific basis for extrapolating from LD/50 levels to carcinogenic, teratogenic, reproductive, systemic, mutagenic, and other types of effects. The cancer-promoting and co-carcinogenic potentials of dioxin and furan compounds which may appear as contaminants in herbicides and the environment demonstrate the folly of such unsubstantiated conclusions. Acute toxicity levels have no bearing on cancer-causing potential at all, unless one were to accept the DEIS's implicit conclusion that the best way to avoid contracting cancer is to get run over by a bus in one's youth.

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See Appendices, Vol. I, pp. 420-485 (EPA guidelines for assessing combined risks of various dioxins and furans).

Impact on Human Health, H. Rep. 78, Subcom. on Nat. Res., Agr. Res., & Env. Com. on Science & Tech., U.S. House of Rep., 98th Cong., 1st Sess. at 47.

The Forest Service has been on notice since EPA's 1979 emergency ban of dioxin-contaminated herbicides that any supposed human toxic thresholds were exceeded in the Siuslaw National Forest. The Forest Service has conducted no research to demonstrate that dioxin levels have receded, and all available evidence suggests that they have increased. Furthermore, the evidence suggests that dioxins and furans have at least additive effects with each other, and synergistic effects with now admittedly possible cancer-initiating agents proposed for use.

The worst-case analysis therefore can not lawfully assume that cumulative incremental effects of the new prescribed fire and spraying program would be insignificant. Nor should any different assumptions apply to any other Forest in the Region, absent hard data to the contrary.

F. NEW DEIS MUST COMPLY WITH CATS DECISION

The DEIS nowhere examines the health and environmental impacts of dioxin pollution, despite the fact that the DEIS mentions in at least two places that dioxin pollution will result from implementation of the program. Available information indicates that there may be significant cumulative exposure to dioxin pollution, thus nullifying the entire risk assessment included in the DEIS. cursory mention of dioxin pollution

resulting from the vegetation management program is not enough. Petitioner Carol Van Strum and her neighbors litigated the subject of dioxin pollution resulting from this same program in 1976. The Forest Service's obligations under the circumstances are a settled matter, are "res judicata," and the Forest Service is therefore collaterally estopped from relitigating issues settled there.^{54/}

Citizens Against Toxic Sprays v. Bergland established a duty for the Forest Service to examine "the hazards to human and animal health posed by . . . TCDD." 428 F. Supp. 908, 926 (D. Oregon 1977). "No subject to be covered in an EIS can be more important than the potential effects of a federal program upon the health of human beings." Id.

The DEIS must include information of vital interest to decisionmakers and to the public, including the extent to which effects of dioxin are unknown and the opinions of responsible opposing scientists, and eschewing the use of conclusory statements about the safety of dioxin exposure. Id. The DEIS must acknowledge that "a threshold limit, below which no toxic

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It is no defense that the Forest Service is not now proposing to use 2,4,5-T or silvex, as in CATS. The proposed Forest Service action may have a significant cumulative impact with its past actions; the Forest Service must therefore must "consider cumulative impacts of the proposed actions which supplement or aggravate the impacts of past, present, and reasonably foreseeable actions." Marsh, 820 F.2d at 1059. This requirement invokes the duties established in CATS.

effects occur, has never been demonstrated for TCDD." Id.^{55/}

The DEIS must also discuss the regulatory history of dioxin, CATS., 428 F. Supp. at 929, include results of EPA monitoring for dioxin, discuss the controversy over the use of TCDD-contaminated Agent Orange in Vietnam while acknowledging that similar human health effects have been reported by residents of the Siuslaw National Forest, discuss effects on industrial workers exposed to dioxin, and discuss thermal conversion of herbicides to dioxin. Id. at 929-931.

The above discussions are mandatory, and the Forest Service can not relitigate the same issues. They are settled between Ms. Van Strum and the Forest Service, which is collaterally estopped from contesting those issues.

H. NEW DEIS MUST ADDRESS THE NINTH CIRCUIT DECISION

The Forest Service's revised DEIS must at least mention the S.O.S./Merrell decision, and -- assuming the Agency might succeed in persuading the courts to reconsider the panel's findings on the research duty -- the Forest Service must still construct worst-case scenarios on the major scientific uncertainties litigated to a conclusion in that case.

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The existence of a "NOEL" for TCDD was recently relitigated by Ms. Van Strum and EPA, and Judge Owen Panner reaffirmed the CATS decision on this issue, based upon all new evidence brought to the record by the parties. Van Strum v. EPA, Civil No. 87-6031-PA (D. Oregon, Opinion of November 3, 1987 at pp. 2-3) (quoting CATS opinion).

1. WCA MUST EXAMINE FIVE RIVERS HEALTH PROBLEMS

Despite the fact that the Ninth Circuit ordered the Forest Service either to obtain the results of EPA's Five Rivers Health Study or to replicate the study retrospectively, the DEIS is simply silent on the subject. In the absence of that missing information, the worst-case analysis ("WCA") must assume that the herbicides applied in Five Rivers in the spring of 1979 caused the catastrophic health problems suffered by Five Rivers residents. The Forest Service may then produce its evidence that such a scenario is improbable.

2. WCA MUST ASSUME THAT SECONDARY INFORMATION IS FRAUDULENT

The Ninth Circuit ordered the Forest Service: (1) to obtain the actual data from laboratory studies the Forest Service relies upon because of "widespread fraud" and invalid data submitted by chemical companies to EPA to support herbicide registrations; and, (2) to ensure the integrity of the data it relies upon. The Forest Service has not done so; instead, its WCA relies almost exclusively on secondary sources of information from EPA. In lieu of obtaining and analyzing the data, the Forest Service WCA must assume that all secondary information from the EPA or the chemical companies relevant to potential human health effects is either fraudulent or otherwise invalid. It can then disclose its evidence that such an assumption is improbable.

3. WCA MUST ASSUME POSSIBLE EFFECTS ON FUTURE GENERATIONS

The Ninth Circuit unambiguously held that an agency proposing the use of inadequately-tested herbicides must address potentially catastrophic reproductive and mutagenic effects by acknowledging "the fate to which it may be condemning future generations."

S.O.S./Merrell, 747 F.2d 1246 n. 9. The WCA "must be bottomed on the assumption that its herbicides are not safe." 747 F.2d at 1244.

Despite an admitted lack of tests on reproductive effects for nearly all herbicides, the WCA nonetheless dismisses any reasonable possibility of reproductive effects on the public, and also blithely assumes that the risk from mutations is no greater than projected risks of cancer.^{56/} These best-case assumptions are unacceptable. The WCA must disclose the potentially catastrophic impact of spreading substances in the environment that may cause birth defects, abortions, reduced learning abilities in offspring, heritable mutations in humans, and mutated cells in disease organisms that can cause epidemics.

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This is preposterous. The risks of cancer are in the risk assessment limited to the generation that is exposed. The risks of mutations are the hazard of mutant alterations to germinal cells that may be heritable. The revised DEIS, if it were to maintain this absurd assumption, should lay some scientific foundation. The mere fact that many mutagens are also carcinogens demonstrates an additive risk; it most emphatically does not limit the mutagenic risk to that of cancer.

III.
CONCLUSION

This DEIS is inadequate to provide a basis for public comment. Only if the Forest Service first seriously attempts to assess environmental consequences can a DEIS approach the detail needed to examine the effects of herbicides and fire. What the public has been telling the Forest Service for several years now is to get the blinders off. Just say the words, "we don't know what the effects will be. It is possible we could harm public health, cause birth defects, cancer, mutations." That is what the courts have required. Only if the Forest Service can admit possibilities can there be a rational decision.

Until the Forest Service as an institution can say such words, there is little hope that the "Herbicide Wars" will end. So long as Forest Service EIS's claim that herbicides are both safe and essential, they will be challenged by the public.

"I believe the industry was beginning to think about changing its forest-management practices at about the time all of this started, back in the mid-1970s. At that time, we were very dependent on herbicides, and we were not as aware of other methods. Now we are finding out that if we do things right initially, if we take care in cutting and harvesting, we don't need to treat the site with chemicals at all. If there had been no challenge, we wouldn't have come to these findings as rapidly."

Siuslaw National Forest Timber Staff Officer John Hoffman, quoted in K. Schneider, The Pesticide Rebellion, Northwest Magazine 10

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(October 7, 1984), Appendices, Vol. I, pg. *419.^{57/}

We and our children have for too long been test animals for government programs using fraudulently-tested chemicals. We will not acquiesce to a government decision that a paltry \$8 million per year of additional profits for the timber barons are more important than the related hazards to us and to thousands of other Northwest citizens.

This draft environmental impact statement must be withdrawn.

for Paul E. Merrell

CAROL VAN STRUM

Paul E. Merrell

PAUL E. MERRELL

^{57/}

Ironically, Mr. Hoffman is the same Forest Service official who swore under oath in Merrell v. Block that the forests would die if herbicides could not be used.

IV. SCHEDULE OF EXHIBITS

Page	Document description
VOLUME 1	
1	J. Feldman, Analyzing the Death of FIFRA Reform, J.P.R., Winter 1987, pp. 23-25.
4	G. Larsen, Summary of efforts to dissolve herbicide injunction.
7	N. Graybeal, July 2, 1987 letter to M. Connelly.
8	M. Connelly, May 26, 1987 petition to N. Graybeal, with attached Forest Service form letter and map.
14	J. Kadera, Forest Service strives to avoid herbicide battle. Oregonian, pg. D-11, June 15, 1987.
16	Merrell v. Block, Civil Nos. 83-3916, 83-3887 (9th Cir.), plaintiff's November 28, 1983 motion to supplement the record, with exhibits.
46	USEPA. Draft Cancer risk-specific dose estimate for 2,3,7,8-TCDD, November 1987, pp. cover, 11-12, 17-19, 36-38.
55	G.L. Kimmel, USEPA, Appendix to above draft cancer risk assessment, Reproductive and developmental toxicity of 2,3,7,8-TCDD (March 4, 1987 review draft).
73	USEPA, National Dioxin Study, August 1987 final report, pp. cover, 74-75.
76	<u>S.O.S. v. Clark</u> , Exhibit B to plaintiff's complaint, cover letter and BLM worst-case analysis, sans appendix.
83	L. Norris, Accuracy and precision of analyses for 2,4-D and picloram in water by contract laboratories. USDA Forest Service, May 3, 1982 Office Report.
114	P. DeFazio, Lane County Commission chairman, February 20, 1985 letter to Hon. T. O'Neill, Jr., with all listed attachments.
196	C. Van Strum, February 22, 1985 letter to Hon. James Weaver, with all listed appendices.

- 248 E. Mrak, May 22, 1986 letter to W. Lockett, re scientific review panel on toxic air contaminants review of dioxin report, cover letter, three pages of attachment.
- 252 O. Hutzinger, direct testimony, Ex. 870, USEPA FIFRA Docket Nos. 415, et al., in re: Dow Chemical, et al.
- 280 USEPA Table of "sampling sites near pulp and paper mills using chlorine", single page giving Region 10 sites.
- 281 L. Belsie, Sizing up the risk of dioxin in paper, Christian Science Monitor, September 25, 1987, pg. 1
- 282 USEPA OPTS, unsigned memo regarding potential dioxin contamination in paper products, with attached R. Kimbrough letter to J. Moore, regarding and including comments on A.D. little risk assessment of dioxins in paper consumer products.
- 347 D. Barnes, USEPA CDWG chairman, October 13, 1987 trip report on 7th international symposium on chlorinated dioxins and related compounds.
- 374 P. Merrell, December 19, 1987 FOIA request to Region VI, Forest Service (copy from Forest Service files), with enclosed R. Crowe, Region VI, January 7, 1988 response letter.
- 385 USEPA, minutes of April 2, 1982 CDWG meeting. pp. 1, 4, 5.
- 388 USEPA OSWER R&D report on health and environmental effects profile for T-, Pe- and He-p-dioxins, January, 1984 draft; pp. cover, 1-8 and 1-9.
- 391 USEPA Dioxin Task Force, September 24, 1981 meeting notes; pp. cover, enclosed ANPR notice, pp. 1-4.
- 396 Dow Chemical Co. Dow Today, July 28, 1983, re: Position strengthened on 2,4-D global sales.
- 398 R. Harless, USEPA. January 16, 1981 report to M. Dellarco, re analyses for di and tetra chlorinated dioxins in 2,4-D.
- 402 USEPA, September 1985 draft work/quality assurance project plan for National Bioaccumulation Study, pp. cover, signature page, table of contents, 41-44, Appendix B, Appendix M.

- 413 K. Schneider, The Pesticide Rebellion, Northwest Magazine, October 7, 1984, pp. cover, 4-6, 8-10.
- 420 L. Thomas, USEPA, Jan. 7, 1987 letter to assistant administrators and other listed addressees, regarding and attaching interim policy for assessing risks of "dioxins" other than 2,3,7,8-TCDD
- 485 Last page of Vol. I.

VOLUME II

P. Mehrle, et al., Toxicity and bioconcentration of 2,3,7,8-TCDD and 2,3,7,8-TCDF in rainbow trout, July 15, 1987 manuscript. for Environmental Toxicol. & Chem.

USEPA, February, 1984 Ambient water quality criteria for 2,3,7,8-TCDD.

VOLUME III

P. Merrell, September 27, 1985 letter to D. Bear, CEQ general counsel, with attachments listed on page two other than the FOIA request, with all exhibits to those attachments.

VOLUME IV

Merrell v. Block, Civil Nos. 83-3887 and 83-3916 (9th Cir.), plaintiffs' excerpts of record.

VOLUME V

C. Van Strum, A Bitter Fog: Herbicides & Human Rights (1983).

VOLUME VI

C. Van Strum & P. Merrell, No Margin of Safety: A Preliminary Report on Dioxin Pollution and the Need for Emergency Action in the Pulp & Paper Industry.

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